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CONTENTS

The Conditioned Motor Reaction: HULSEY CASON 153

An Instrument for Measuring Certain Aspects of Intelligence in Relation to Growth, Practice, Fatigue and Other Influences: E. L. THORNDIKE . . 197

Reliability of [illegible] Soundness Tests: J. O. HOLSOPPLE 203

The Reliability of the Seashore Phonograph Record for the Measurement of Pitch Discrimination: A. R. GILLILAND AND C. R. JENSEN 214

The Stimulus Error: A. P. WEISS 223

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THE CONDITIONED EYELID REACTION

BY HULSEY CASON

COLUMBIA UNIVERSITY

SECTION I

Introduction

The present experiment on the conditioned eyelid reaction should probably be considered in connection with a former one on the conditioned pupillary reaction (4). An effort was made in both cases to eliminate voluntary factors as far as possible. In the pupillary experiment, no measure was taken of the time which elapsed between the reception of the conditioning stimulus and the pupillary reaction. This was hardly necessary since the pupillary reaction is not subject to voluntary control in a strict sense. The pupillary reaction is a relative affair. The eyelid reaction, on the other hand, can be controlled voluntarily to some extent, and is generally 'all or none.' In the present experiment a measure was taken of the time which elapsed between the reception of the conditioning stimulus and the eyelid reaction. The ability of the subject to react somewhat after the manner of the usual reaction-time experiment was also measured. These latter measurements were taken in order to determine the speed with which the subject was *able* to wink when the sound stimulus was given. While the time factor is not a sure criterion for distinguishing a 'reflex,' it is clear that if the time of the conditioned eyelid reaction is made faster than voluntary factors can make it, there is good reason for believing that 'voluntary' factors do not by themselves account for the result.

In the conditioning process, winking was effected by the electrical stimulation method, the 'active' electrode being placed just under the right eye of the subject. These electric shocks produced winks in a very regular manner. The conditioning stimulus was a sharp, sudden sound, of *low* intensity, which was found by trial not to be sufficient to call out a reflex wink at the beginning of the experiment. The subject was given the electric shock and made to wink simultaneously with the sound a large number of times. The sound stimulus was then given alone, without the electric shock, and the time of the wink, if such occurred, was measured. In most cases the conditioned eyelid reaction which was secured was faster than the normal reaction time to the same stimulus. After the sound stimulus and the shock (which caused the wink) had been repeated together a thousand or so times, it was found that the difference between the time of winking to the compound sound-shock stimulus and to the shock stimulus alone was greater than at the beginning of the experiment. The presence of the sound now apparently increased the speed of winking more than it had done before. When the shock was now given alone, there were irregularities in the winking, which were not present at the beginning of the experiment. Apparently some connection had been formed between the sound stimulus and the winking reaction.[1]

Section 2

The Eyelid Reflex

The following is a brief statement of some general features of the eyelid reflex, together with a summary review of the methods which have been employed in investigating this reflex. This will aid perhaps in a better understanding of the various factors encountered in this particular experiment.

Of the two lids or palpebræ of the eye, the upper is the larger and more movable, and has its own levator muscle, the levator palpebræ superioris, which is supplied by a branch

[1] The writer is under many obligations to those who were kind enough to serve as subjects, especially to Miss Eloise Boeker. I wish also to express my appreciation to Professors R. S. Woodworth and A. T. Poffenberger, and to Mr. Arthur L. H. Ruben for much valuable assistance.

of the 3d nerve. The lower lid is not very mobile and lacks a depressor. Closure of the lids is effected by a ring of smooth muscle fibers, the orbicularis palpebrarum, which is common to both lids, and is innervated by the 7th nerve (3, 6).

Closure of the lids may occur: if a very bright light enters the eyes, by the sudden approach of some foreign body, by contact of a foreign body with the lashes, and by irritation of the cornea or conjunctiva (21, 514). "The reflex can be initiated by the stimulation of any of the branches of the ophthalmic (1st) division of the 5th (trigeminal) nerve. From the nucleus of this nerve in the pons Varolii fresh fibers take the impulses, it is believed, to the upper part of the facial nuclei of both sides (7th nerve), and from these to the orbicularis palpebrarum. This reflex is one of the last to be abolished by anæsthetics . . . " (21, 514). It is variously named the palpebral reflex, the conjunctival reflex, or the corneal reflex (17, 12). According to Potter (19, 294), the nerves concerned are the 3d, facial, and sympathetic to the muscles; and the 5th to the skin and conjunctiva.

The wink reflex is one of the most constant and fastest of human reflexes, and it has been studied by a variety of methods. In 1874, Exner (11) measured the time of winking by means of a long, light lever, which was fastened to a frame and held firmly between the teeth. A thread was attached to the lever near its pivoted end. This thread passed over two pulleys and was glued to the eyelid. The lever wrote on a revolving drum. An electrical shock was applied to the lower eyelid to cause reflex winking. The current was varied by changing the distance of the secondary coil of an induction apparatus from the primary coil. Exner found that the reflex could be made faster by increasing the current. With the secondary coil at a distance of 9 cm., the average time was 0.0662 sec., when at a distance of 5 cm., the average time was 0.0578 sec. This time includes the time of transmission of the nerve impulse, the latent period of the muscle, and also the time taken by the nerve cells in the central nervous system. Exner made an estimate of the 'central

time,' but it does not seem advisable to give his figures since the speed of nerve transmission and also the latent periods of the muscles involved are not definitely known (16, 35–36; 15). Exner does not state what strength of electric current was used, and this is known to influence the speed of the wink. According to Stiles, "When the lower eyelid receives an electric shock of sufficient strength there is a wink which is quite involuntary. This is a reflex and is accomplished within 0.05 second of the stimulus. With a somewhat weaker shock it is often found that the delay is three times as long. Such a case is a voluntary reaction; there is a sensation, a purpose to wink, and then the movement" (22, 58–59).

Mayhew's method (16) had some points of superiority over that used by Exner. He had a fine copper wire connected with a small piece of platinum foil, which was itself fastened to the eyelid and also insulated from it. On a light aluminium frame fitted around the subject's head was mounted an adjustable bit of platinum wire, in such a way as to touch the foil on the eyelid when the latter was open. A slight movement of the eyelid broke the electrical circuit. The lid reflex was called out by applying a light blow to the face about an inch below and to the outer side of the outer canthus of the eye, on the side from which the record was taken. This was executed by means of a small spring hammer controlled electrically. Mayhew does not say with what force the blow was struck. As the eyelid follows the vertical movements of the eye to some extent, Mayhew had his subjects fixate on a certain object. The record was taken on a kymograph, and timed with a tuning fork. From Mayhew's results of 450 'experiments' on 16 subjects, with the same stimulus but under different conditions, I find the average to be .0413 ± .0001 sec. The S.D. is .005 sec. This average figure represents total time, and is less than that given by Exner. The two experiments, however, were carried out under somewhat different conditions.

Mayhew attempted to test the effect of different mental states on the wink reflex. Some subjects were required to do mental multiplication; some were required to keep the

mercury in a manometer at a certain height; while others were warned a few seconds before the blow that it was about to fall. He did not get very regular results. The plotted curves of different subjects did not correspond to one another; but for any given individual there was a close correspondence between the averages of sets of experiments obtained under similar conditions. In those experiments, however, in which apprehension was felt, the average time was found to be shorter (16, 46). The reflex time varied in different individuals, the average times (he does not state for how many cases) falling between 0.0351 and 0.0491 sec.

Garten (13) was the first to use the photographic registration method in studying the wink. He studied the voluntary wink and also the reflex wink. Weiss (25) also used the photographic registration method, but only studied the voluntary wink. Weiss found that the course of a single voluntary wink, in his own case, was divided as follows: closing the eyelid took on the average .060 sec., ranging from .043 to .107 sec.; the lid remained closed an average time of .031 sec., ranging from .014 to .055 sec.; opening the eyelid took on the average .111 sec., ranging from .077 to .160 sec. (cf. 12; and 13).

Dodge (7, 41–45) used his photographic registration method to measure the latent time of the protective wink. The head of the subject was placed in such a position, between an arc light and the photographic recording apparatus, that a shadow of the eyelash fell perpendicularly across the slit. Dodge employed the sound of a clapper on a sounding-board to call out the reflex wink. For 4 of his human subjects, the latent times were 36, 42, 42, and 56σ. Dodge's method is to be recommended wherever very accurate measurements are desired.

Judging from 26 cases, Mayhew concluded that a natural wink, occurring very shortly before a second wink was called out, had no influence (16, 44). According to Sherrington, "In the eyelid-reflex for nearly a full second after initiation of a reflex, the chance that a second stimulus then delivered will, though otherwise appropriate, excite the reflex, is fifty

per cent less than it is one second later. The refractory phase, therefore, is marked though not absolute: it operates longer for a visual stimulus than for a tactual or thermal" (20, 45).

Dodge has studied the refractory phase of the wink reaction. He states that "Stimuli from 500σ to 700σ apart gave double reactions in 34% of the cases. Stimuli from 750σ to 1000σ apart gave double reactions in 67% of the cases. Stimuli from 1000σ to 1250σ apart gave double reactions in 100% of the cases" (8, 6; see also 26).

A brief review of Partridge's work will aid us in understanding some further factors which are encountered in this experiment. Partridge (18) studied the ability of normal people to inhibit the reflex wink. Close before the subject's face was placed a framed piece of thick plate-glass. "On the back of this glass and attached to the lower side of the frame was a small rubber-faced wooden-headed hammer which, when released from a catch under the control of the experimenter, was swung suddenly upward, and struck the glass about the level of the eyes of the subject. . . . " (18, 244). In the experiments with the university men, Partridge found the greatest difference in the power to inhibit the wink. In the case of one subject, who was given a series of 20 tests of 50 trials each, on alternate days, the number of winks inhibited each day was as follows: 0, 0, 0, 0, 5, 6, 0, 6, 11, 5, 10, 10, 17, 17, 32, 21, 21, 15, 26, 16, 41, 16, 22, 23, 15, 27, 21, 32, and 35. It seemed to this investigator that a change in the rhythm of releasing the lever or a short interruption in the series lessened the control in the case of this subject. Fatigue also seemed to lessen the power to inhibit the wink. Partridge similarly tested 1100 pupils of the Worcester schools (18, 247). Improvement was more marked and also more uniform for boys than for girls. A boy described by his teachers as 'nervous' was more apt to have difficulty in controlling the wink, but a girl so described was "but little more likely to have difficulty than one described as normal" (18, 249). "Momentary changes in attention frequently inhibited a few winks in a series long

before control was finally gained. Smiling seemed to be especially effective in temporarily inhibiting the reflex. Changes of any kind in the environment, as of some one entering the room, opening a door, an unexpected sound, usually increased the tendency to wink, though in some cases such a distraction seemed to have the opposite effect" (18, 250).

Section 3

Electrical Stimulation of Muscles and Nerves

There are a few facts about the electrical stimulation of muscles and nerves which should be kept in mind; and while these are being briefly stated I should like to add along with them certain facts about the procedure which was used in this experiment.

The eyelid reflex was evoked by electrical stimulation, the current being furnished by an induction coil. The 'indifferent' electrode consisted of a cup of salt water, into which the three middle fingers of the subject's right hand were placed. This made a very constant contact. The 'active' electrode, where the effect of the current was more pronounced, consisted of a thin sheet of aluminium, 1.5 by 2.5 cm., placed over the levator labii superioris muscle and about 1 cm. above the zygomaticus major muscle of the subject's face (see the charts of motor points of Erb, 10, 289; Castex, 5, 371; and especially Tousey, 24, 386). The active electrode was held in place by adhesive plaster. I tested out various positions around the eye for this electrode, and it seemed that this particular position was most effective in calling out the wink, although a certain amount of squinting was also present.

The electric shock used in calling out the wink reflex may have stimulated the facial nerve directly, and it may have also had an indirect reinforcing effect through stimulation of the ophthalmic division of the trigeminus nerve. According to Du Bois-Reymond, the effect of such stimulation is proportional to the amperage which actually reaches the muscle or nerve, and to the rapidity of the change in

the strength of the current (24, 303). This law is not rigidly correct (14, 130–131), but is useful for most practical purposes. Certain further facts about electrical stimulation have a bearing on our experiment, and they might be very briefly mentioned. The best effect is produced by placing the active electrode directly over the motor point of the muscle. A striated muscle is affected most at the cathode when the current begins, and at the anode when the current is turned off. The wave of stimulation in a nerve starts from the cathode when the current is turned on or its strength is increased, and from the anode when the current is turned off or its strength is diminished. The cathodal closure effect is stronger than the anodal closure effect. In this experiment with the eyelid reflex, the cathode was the active electrode; but it makes little difference when a faradic current is used. The current I employed was not strong enough to stimulate the orbicularis palpebrarum muscle directly, since a period of only about 2σ elapses between the direct application of an electrical stimulus to a muscle and the muscular contraction (24, 329). The time measures I obtained were considerably longer than this figure. If the stimulus is applied to the nerve, in order to affect some muscle, then the time of nerve transmission must be added. Mayhew gives the distance from the eyelid to the center controlling the wink and return as roughly 35 cm. (16, 35–36). The fatigue effect of continued stimulation, which has not been accurately determined, was probably due more to nervous than to muscular fatigue (24, 333). This is especially true when an induced current is used (24, 343). Any reduced response to the successive stimuli is probably entirely due to the phenomenon of fatigue rather than to the stimulation of inhibitory nerves which do not exist in the ordinary voluntary muscles.

Section 4

Apparatus

All time measurements were taken with the Bergstrom chronoscope. This apparatus will not be described since a

good description can be easily obtained (see 1 and 2). All measurements were made with the weight at the upper end of the pendulum taken completely off. While this instrument is not as precise as the Hipp chronoscope, it was better adapted to the purposes of the present experiment. The Bergstrom chronoscope had the following advantages over the Hipp chronoscope: (*a*) measurements could be made much faster, (*b*) the disturbing sound of the Hipp chronoscope was not present, and (*c*) the electrical wiring was greatly facilitated.[1]

[1] As a check on the accuracy of this Bergstrom chronoscope, it was decided to connect it with a properly timed Hipp chronoscope and make several measurements on the two simultaneously. For assistance in making the electrical connections I am indebted to Mr. H. K. Nixon. I have also to thank Mr. J. L. Holmes for the use of the Hipp chronoscope which he was using in a reaction-time experiment. The main features of the electrical wiring were as follows:

Pressure on a telegraph key closed the circuit passing through the pendulum release of the Bergstrom chronoscope, starting the pendulum; and at the same time operated a relay. This relay broke the circuit in the upper coils of the Hipp chronoscope, which allowed the armature to fall, and started the clock. The telegraph key was quickly released, so as to put the current back in the upper coils of the Hipp chronoscope. The two clocks were stopped by pressing a second telegraph key. This was used to close the circuit passing through the electromagnets of the Bergstrom chronoscope, which stopped the swinging pointer; and at the same time operated a second relay. This relay broke the circuit passing through the lower coils of the Hipp chronoscope, stopping the clock. In this way both chronoscopes were started and stopped together. The only latent time in the arrangement, which was not ordinarily present in the two chronoscopes, was that of the relays. The armature connections of both relays were normally closed by the light springs. A fraction of a sigma may have elapsed before the inertia of the relay armature was overcome, and before it could be pulled away from the contact. The latent time of the relay in the starting system, however, almost exactly balanced the latent time of the relay in the stopping system. It was not necessary to interfere in the least with the 4-volt current ordinarily used for the Hipp chronoscope; and the currents employed to start and stop the Bergstrom chronoscope were so regulated that they were exactly the same as during the wink experiment.

By this arrangement I started and stopped both chronoscopes 500 times, taking readings on each chronoscope at each trial. I tested out the scale on the Bergstrom chronoscope from 50σ to 1,000σ, taking most of the readings in the range from 80σ to 500σ. The measures showed a constant error on the Bergstrom chronoscope of about 15σ. Its readings were too large when compared with the Hipp chronoscope. I next made a scatter diagram of the 500 pairs of readings, representing each pair by a dot. A curvilinear line was drawn freehand through this very long and narrow distribution, an effort being made to keep the sums of the squares of all the perpendicular distances from the dots to the curvilinear line as small as possible. Then I made a table of the coördinates of this curvilinear line, at intervals of 1σ, and used this table

I used three different sound stimuli at various times. The loudest sound was produced by a telegraph sounder, which was mounted on a box and situated about 6 in. from the subject's head. This sound was intended to call out the wink reflexly, but it did not succeed in all cases. This sound was only used for a few of the first experiments performed. Definite mention is made of the fact in this report whenever it was used, and the measures of the time of winking made with it are referred to specifically as 'sound reflex' times. It was never used as the conditioning stimulus. Another sound stimulus was produced by a second telegraph sounder, located at a distance of some 5 ft. from the subject. The amplitude of this sounder was made less than the other, and the sound intensity thereby decreased. The third sound, which was used in the case of most of the subjects, was the click of a relay, whose armature had an amplitude of some 2 mm. In the case of both the relay and the sounder, one side was deadened so that only a single click, instead of a double one, could be heard.

The device used for registering the time of the wink was a thin aluminium lever attached at one end to the eyelid, and supported just to the right of the subject's right eye by a pair of 'spectacles.' The 'spectacles,' which were made of heavy wire, 3 mm. in diameter, could be bent to conform to different subjects. Once properly fitted, they remained in the same relative position, and were not uncomfortable.

to transmute Bergstrom readings into Hipp readings. The S.D. of the Bergstrom readings, taking the Hipp as standard, was found to be about .004 sec. for that part of the scale with which we are most concerned. The S.D. of the Hipp readings, however, when compared with a fall apparatus, was known to be about .0007 sec. Hence the S.D. of the Bergstrom, if compared with a standard which did not itself vary (*i.e.*, if compared with the 'true') would be *less* than .004 sec., being given by $\sqrt{(.004)^2 - (.0007)^2}$, or .0039 sec. If a time function is now measured by the Bergstrom the obtained S.D. of the distribution will be too high, because the Bergstrom is itself varying while the measurements are being made. If the obtained S.D. of a distribution of wink times, for example, measured by the Bergstrom, is found to be .009 sec., then the true S.D. of this distribution would be given by $\sqrt{(.009)^2 - (.004)^2 - (.0007)^2}$, or .008 sec. This is mentioned to show that my obtained variabilities are, if anything, given too high. I will not make use of this correction, however, in the computations to follow, because I have no good reason for believing that variations in either of these chronoscopes follow the normal curve of distribution. Still, it must be admitted that the *most probable* variability would be obtained only by applying the correction.

The aluminium lever was 0.6 mm. in diameter and 10 cm. in length. One end was drawn to a point and attached to the subject's eyelid by means of a piece of sticking plaster, 2 by 7 mm. The sticking plaster was placed on the subject's right eyelid, just above the free margin. The point of the lever was passed through the central portion of this strip of sticking plaster and back again. There was no danger of the lever coming loose, and very little chance for the sticking plaster to come off of the eyelid. The lever was free to slide to the right and left in a fulcrum which could be adjusted to a favorable position. The lever was very delicate, and so balanced that after a few minutes the subject was no longer conscious that anything was moving with his eyelid. To the outer end of the lever (the end away from the subject's right eye) was attached a piece of German silver wire. The outer end of the lever was about 2 mm. below a second piece of German silver wire when the eyelid was in the normal raised position. Whenever the subject winked the outer end of the lever was raised and was brought in contact with the second piece of German silver wire. This caused an electrical contact to be made, every time the subject winked. The distance between the lever and the stationary wire was maintained practically constant, so that the contact was made with the eyelid always in the same 'phase.' This last adjustment was necessary in order to secure a fairly high degree of accuracy. This apparatus has very distinct advantages over the other devices which have been used. It is very simple in mechanism, and does not often get out of order. It allows the subject to move his head freely, without any interference in the functioning of the device. It can be worn for a continuous period of 4 hours without any particular discomfort. It is very accurate, and enables the wink to be registered when the eyelid is in practically the same position each time.

The electrical contact, which was made when the eyes were closed, was used to operate a relay, which had a very small 'amplitude.' When the relay was closed by a wink the current from 6 dry cells was circuited through the coils of

the relay, keeping it closed, and at the same time stopping the pointer on the Bergstrom chronoscope. The pointer was held in the clamped position even though the contact at the eye was almost immediately broken. After noting the time, I would quickly release the pointer by a convenient switch, the purpose being to keep the 6 dry cells from 'running down.'

The apparatus used in registering the time of the wink, the manner of evoking it reflexly, and the various sound stimuli employed, have now been described in detail. Certain electrical connections and combinations of stimuli used at different times are as follows:

1. One side of a double knife switch was employed to release the pendulum, the other side being used to give the shock. Thus the pendulum could be released and the shock given at the same time, and the time which elapsed between the *shock* stimulus and the wink reaction could be measured.

2. Closure of a telegraph key could release the pendulum, produce any one of the sound stimuli, and give the shock, all at the same time. In this way the speed of winking to the *shock and sound* together could be measured.

3. The shock could be cut out of combination 2 by opening a single switch; and the time of winking (if such occurred) to the *sound alone* could be measured.

4. During the 'training series,' which consisted of a great many repetitions of the shock and sound together, I did not care to take the time measurements of every one of the winks to this combination. While no time measurements were being taken the electrical connection to the pendulum was short-circuited, and the pendulum remained in the set position. To measure the time of winking to the sound alone, it was now only necessary to break the circuit involved in the shock stimulus, open the switch short-circuiting the pendulum, and press the key. The time of winking to this sound, if a wink occurred, would represent conditioned reaction time. These switches were manipulated noiselessly, and at irregular times; so that the subject was almost always taken by surprise.

Section 5

Procedure

The procedure was varied somewhat at different times, but the general plan used in the case of the first 7 subjects was about as follows:

1. The wink-registering apparatus was placed on the subject, and properly adjusted. He was next introduced to the shock, which was made just strong enough to call out the wink reflexly.

2. Without giving any instructions I repeated the sound stimulus alone several times. I made sufficient trials to be sure that the sound did *not* cause a wink at the beginning of the experiment.

3. I next alternated with measurements of the time of winking (*a*) to the shock alone and (*b*) to the shock and sound together, taking 20 or 25 of each. I thus had a measure of the effect produced by the shock, both with and without the sound, before the long training period was begun.

4. The training series consisted of from 500 to 3,000 repetitions of the shock and sound together, given at intervals of from 3 to 6 seconds. After this compound stimulus had been repeated from 15 to 35 times (the number was purposely varied, but averaged about 25), I would cut out the shock and give the sound alone one time, to see if a conditioned reaction could be evoked. Proceeding in this way, I obtained several measures of the conditioned reaction time.

5. During the training series, I would also take an occasional measure of the speed of winking to the shock and sound together, for use in comparisons.

6. After the training period was over, and after the measures of the conditioned reaction had been made, I again mixed up measurements of the time of winking (*a*) to the shock alone and (*b*) to the shock and sound together. I had now a measure of the speed of winking to the shock, *with* and *without* the sound stimulus, both *before* and *after* the 'training.'

7. The connection which had been established between

the eyelid reflex and the sound stimulus was 'unlearned' by repeating the sound alone a few times.

8. The subject's voluntary reaction time to the sound stimulus was now measured. He was instructed to listen attentively for the sound stimulus and to wink as quickly as possible when it was heard. No 'ready' signal was given before each reaction. This 'ready' signal was omitted so that the conditions here would be more nearly like those under which the conditioned reaction time was measured. In this way a comparison could be made between the conditioned reaction time and the voluntary reaction time. The voluntary reaction time showed the speed with which the subject was *able* to wink to this same sound. It should be pointed out that no instructions at all were given when a 'conditioned reaction' was called out. In the voluntary reaction time, however, he was definitely instructed to wink as quickly as possible.

9. In a few cases, winks were called out reflexly by the rather loud stimulus, which was produced by a telegraph sounder, mounted on a box, 6 in. from the subject's head. I was trying to make a fair comparison between this 'sound reflex' time and the conditioned reaction time. But it seems that a just comparison cannot be made; since the reflex winks to the loud sound were somewhat irregular for all subjects; and, besides, the two stimuli were themselves *different*.

There is one difficulty with the procedure, as outlined above, which apparently cannot be surmounted. The subject was of course winking voluntarily off and on during the experiment. An effort was made to give the stimuli at times when the subject was not in the act of winking; and also when he had not just completed a wink. Every time the subject winked a relay made a faint sound, distinguishable by me, but not heard by the subject. This was much better I think than looking at the subject's eye all the time. I could tell whenever his eye closed, and gave the stimuli at what seemed to be appropriate times. I could easily avoid giving the stimuli immediately after a natural wink; but

could not avoid giving the stimuli sometimes when the wink had already been initiated. If the wink was already 'on the way,' the registered time would be too short; and if a wink had just been completed, the second wink would probably be delayed.

Section 6

Results

Since the detailed procedure was changed from time to time, the results from all subjects cannot be readily combined. The changes in procedure had to do principally with the kind of sound used for the conditioning stimulus, with the number of repetitions of the training stimuli between each measure of the conditioned reaction, and with the particular time of taking the reaction time to the sound stimulus. The results of the different subjects will be described separately, in the order in which the trials were made. This will show the different effects which may be produced by certain changes in procedure.

Table I. gives a summary of the results obtained with subject *J. H.*[1] The main comparison we wish to make in this particular experiment is between the 'conditioned' reaction time and the voluntary reaction time:

	C.R.	R.T.
Av.	173.5 ± 3.07	228.7 ± 10.47
Med.	160.0 ± 3.85	195.0 ± 13.12
R.T.—C.R. (Av.)	+ 55.2 ± 10.91	Reliability = 99.996/100
" (Med.)	+ 35.0 ± 13.67	" = 95.73/100

These figures suggest that the conditioned reaction time was faster than this subject's voluntary reaction time. Al-

[1] The number of measures taken in each of the 9 distributions is given in this table, together with the Av., P.E. of the Av., S.D., Q_1, Med., P.E. of the Med., and Q_3, for each distribution. The S.D., which was calculated from the Av., was used throughout in calculating the P.E. of the Med. The medians of the various distributions are given along with the averages, but the average is the better measure in practically all cases both because of the mathematical development of certain formulæ and also because there is no very good reason why the extreme measures in this particular case should not be weighted. The extreme measures were mostly accidental. The median has only a limited meaning; whereas the average is more of a mathematical expression. In all of these computations, I make no correction for the variability in the instruments, in calculating the standard deviations. My variabilities are obviously given somewhat too high in all cases.

TABLE I

SUBJECT *J. H.*, JAN. 28, 2:30 P.M.–5:00 P.M.

	Sound Reflex (Involuntary)	Shock	Shock and Sound	C. R. (Sound)	Shock and Sound	'Unlearning'	R. T. (Voluntary)	Sound Reflex (Involuntary)	Shock
Meas.....	21	21	21	13 (last)	26	15	19	11	11
Av.....	127.3±1.25	128.6±1.50	132.6±2.06	173.5±3.07	140.1±1.38	250.9±.18.97	228.7±10.47	213.6± 9.78	158.2±3.09
S.D.....	8.5	10.2	14.0	16.4	10.4	108.8	67.7	48.1	15.2
Q_1.....	119	120	121	143	136	173	179	172	147
Q_2.....	126.0±1.57	128.0±1.88	128.0±2.58	160.0±3.85	140.0±1.73	189.0±23.78	195.0±13.12	206.0±12.26	153.0±3.87
Q_3.....	134	138	140	170	147	303	290	260	170

The numbers in the above table represent the time in sigma which elapsed between the reception of the stimulus and the winking reaction. The order of the experiment was from left to right, that is to say, the 'sound reflex' times were taken first (1st column), next followed the time of winking to the shock alone (2d column), and so on. The subject did not wink to the 'conditioning' stimulus, the sound of a *relay*, at the beginning of the experiment. The sound reflex (1st column) was called out by a loud telegraph sounder. Column 2 in the table shows the time of winking to the shock alone; while the third column shows the time of winking to the sound of the relay and the shock together. Each conditioned reaction (C.R.), *i.e.*, winking to the sound alone, was preceded on the average by 25 repetitions of the 'training stimuli,' the shock and sound together. The conditioned reaction was evoked 17 times out of 24 attempts. There were 600 repetitions of the training stimuli; and the time of winking to each of these was not measured. To *unlearn* the conditioned reaction, the sound was simply repeated alone several times. The first four measures of the 'Unlearning' time were 166σ, 173σ, 174σ, and 189σ. After this point the reactions were long and irregular. The time required to react voluntarily to the relay was taken without a 'ready' signal: the subject was simply instructed to wink as quickly as possible every time the sound was heard. This subject was not feeling at all well during the experiment.

though there was a general slowing up of all times during the experiment, the reaction time of this subject was probably *faster* at the end than it 'would have been' at the beginning. This matter was tested out in the case of the next three subjects to be considered, where the conclusion is confirmed. This experiment therefore seems to indicate that the eyelid reflex *can* be conditioned to a sound, and that the speed of the conditioned reaction may be made fast enough to rule out the possibility of voluntary reaction. In voluntarily winking to a sound, it is possible that a real inhibitory effect might have been present, due to the subject holding the lid in a 'state of readiness.' That is to say, the subject might have been 'holding' his eyelid in the open position, in waiting for the stimulus, so that a short interval elapsed before he could release this 'hold' and innervate the antagonistic muscles. The *same* consideration, however, would obtain where a measure is being made of the conditioned reaction time, in case the subject did try to react voluntarily to the 'conditioning' stimulus. There is no good reason for believing that this 'inhibitory effect' behaved differently in the two cases.

It seems plausible that the subject might have been in a state of continued apprehension during the training series, due to receiving the electrical shock so many times and in such an irregular way. Some measure of this factor, however, was obtained with later subjects as it does not seem that it is sufficient to explain the results.

The sound reflex time is a hybrid measure, mainly because the loud sounder did not produce a reflex wink in all cases.

Table II. shows the results in full for subject *A. R.* This subject's voluntary reaction (winking) time to the sound stimulus was definitely faster at the end than at the beginning of the experiment. The average reaction time at the beginning of the experiment was 285σ; and at the end it was 203σ. It is probable that the speed of winking voluntarily was continuously improved throughout the experiment, but the exact shape of the curve is not known. There is of course a possibility that the curve took a rather sudden drop when

TABLE II

SUBJECT *A. R.*, JAN. 30, 10:30 A.M.–2:30 P.M.

Sound	R. T.	Shock	Sound and Shock	C. R. (Sound)				Sound and Shock	R. T.			Shock
—	224	126	123	—	—	234	176	126	348	147	152	126
—	211	122	117	—	333	89	209	126	160	296	195	130
—	293	121	127	333	422	72	176	134	143	147	217	130
—	268	114	122	—	224	234	194	122	103	184	217	130
—	186	117	122	—	277	204	184	127	130	151	242	122
—	381	130	125	715	72	—	234	123	143	214	202	130
—	366	122	119	—	—	209	176	—	107	355	155	136
139	122	127	122	—	—	277	138	122	155	160	155	143
—	245	105	122	—	277	107	305	112	151	199	157	135
—	333	118	121	—	234	—	72	122	225	172	252	143
—	352	126	124	—	184	214	348	134	153	240	160	130
—	167	107	120	862	42	296	160	119	167	160	179	—
—	252	118	111	287	—	214	315	117	270	194	151	122
—	651	118	118	—	—	209	204	126	260	168	192	135
—	298	113	119	42	—	292	211	112	210	194	176	—
—	315	112	117	266	199	84	154	111	237	176	168	—
—	324	122	125	395	138	189	—	112	160	216		144
—	196	126	122	—	204	168	234	108	122	224		136
168	224	106	120	209	194	124	103	109	219	204		134
—	297		130	—	84	434	—	112	250	298		130

TABLE II. (*Continued.*)

SUBJECT *A. R.*, JAN. 30, 10:30 A.M.–2:30 P.M.

Sound	R. T.	Shock	Sound and Shock	C. R. (Sound)				Sound and Shock	R. T.		Shock
—	251			—	199	151	147		209	324	
—	239			—	277	126	199		172	184	
—	265			—	42	204	310		191	178	
—	422			324	310	199	168		273	164	
—	250			209	—	199	138		134	170	
				395	260	229	199		144	277	
				42	84	72	385		225	204	
				266	348	348	214		161	324	
				58	—	209	234		160	296	
				72	189	—			168	244	
				—	194	184			127	155	
				255	—	266			155	202	
Meas. . .	25	19	20	79 (last)				19	48 (last)		17
Av.	285.3 ±13.42	118.4 ±1.13	121.3 ±0.51	205.2 ±6.28				119.7 ±1.22	203.0 ±5.09		132.7 ±1.05
S.D.	99.5	7.3	4.0	82.8				7.9	52.3		6.4
Q_1	224	113	119	154				112	162		130
Q_2	265 ±16.82	118 ±1.42	122 ±0.64	199 ±7.87				122 ±1.53	193 ±6.38		130 ±1.32
Q_3	324	126	123.5	234				126	220.5		136

In the above table the experiment proceeded from top to bottom in the columns and from left to right on the page. The conditioning stimulus was a telegraph sounder, which did not call out a reflex wink at the beginning of the experiment. There were 3,125 repetitions of the training stimuli (shock and sound together), given at intervals of from 3 to 10 sec. 125 attempts were made to call out the conditioned reaction; each one being preceded on the average by 25 repetitions of the shock and sound together. A dash in the table indicates that the subject did not react at all within 1 sec.

the electric shocks were first given, so I am on the safe side in using the voluntary reaction time which was obtained at the *end* of the experiment in all comparisons.

This is the only case where the conditioned reaction time is at all *slower* than the voluntary reaction time:

	C.R.	R.T.
Av.	205.2 ± 6.28	203.0 ± 5.09
Med.	199.0 ± 7.87	193.0 ± 6.38
R.T.–C.R. (Av.)	− 2.2 ± 8.08	Reliability = 57.62/100
" (Med.)	− 6.0 ± 10.13	" = 65.50/100

The difference, C.R.–R.T., however, is not reliable. The possibility of a voluntary reaction is not eliminated in this case. It is well to point out that the subject was told to react as quickly as possible when the voluntary reaction time was measured; whereas he was given no instructions whatever when a measure of the conditioned reaction time was taken, and he had no particular 'will to wink' in the latter case. He did not wink reflexly to the sound stimulus at the beginning of the experiment, as the first column of the table shows. The conditioned reaction naturally did not function well at the beginning of the training series (see columns under C.R. in Table II.), but as the training was continued the conditioned reaction became more and more regular, and eventually seldom failed to function. The very marked irregularities in the time of the conditioned reaction can only partly be explained by the fact that the subject was winking off and on during the experiment. There are various factors which would naturally tend to make the conditioned reaction irregular and halting. For example, it is a newly acquired reaction, and the stimuli were given in an irregular manner. These factors, however, do not excuse the rather marked irregularity in the conditioned reaction times, which is for the most part very little less than that of the voluntary reaction time. Our conditioned reaction seems to have little of the regularity and constancy which is very characteristic of some 'reflexes.' It is obviously impossible to treat statistically those trials when the subject did not react at all.

In the case of subject *E. B.*, Table III., the voluntary

TABLE III

SUBJECT *E. B.*, JAN. 31, 10:10 A.M.–1:30 P.M.

Sound	R. T.	Shock	Sound and Shock	C. R. (Sound)			Sound and Shock	R. T.	Shock
—	333	122	130	—	—	234	130	170	123
—	385	122	126	277	—	—	—	—	122
—	336	122	131	—	—	191	138	—	—
—	260	116	138	—	—	—	132	355	140
—	334	118	130	—	234	—	114	—	—
—	195	132	132	—	199	—	130	260	148
—	455	124	136	—	—	233	131	252	142
—	290	124	140	—	—	176	—	—	146
—	284	117	117	310	203	58	113	244	147
—	230	113	135	—	—	—	115	235	145
—	278	130	135	—	—	42	—	296	139
—	261	136	134	—	—	—	117	220	165
—	311	118	130	—	—	—	121	277	121
—	302	116	127	271	—		125	245	155
—	309	122	125	—	—		130	300	144
—	240	122	120	—	42		115	218	143
	266	136	134	—	168			194	124
	199	112	134	—	465			168	122
	349	139	115	—	—			—	161

TABLE III. (*Continued.*)

SUBJECT *E. B.*, JAN. 31, 10:10 A.M.–1:30 P.M.

Sound	R. T.	Shock	Sound and Shock	C. R. (Sound)		Sound and Shock	R. T.	Shock
	232	131	123	—	260		271	—
	227	123	115	—	—		—	168
	326			—	—		258	131
	297			58	—		258	121
	215			255	385		247	
	252			—	—		233	
				—	—		189	
				229	229		266	
				135	—		200	
				—	40		306	
				130	—		265	
				—	42		277	
Meas.....	25	21	21	22 (last)		13	25	20
Av.....	286.6 ± 8.00	123.6 ± 1.12	128.4 ± 1.09	182.2 ± 15.66		123.9 ± 1.48	248.2 ± 5.80	140.3 ± 2.20
S. D.....	59.3	7.6	7.4	108.9		7.9	43.0	14.6
Q_1.....	240	118	125	58		115	220	123.5
Q_2.....	284 ± 10.03	122 ± 1.40	130 ± 1.37	195 ± 19.63		125 ± 1.85	252 ± 7.27	142.5 ± 2.76
Q_3.....	326	130	134	234		130	271	147.5

In the above table the conditioning stimulus was the click of a relay. This did not call out a reflex wink at the beginning of the experiment. There were 1,875 repetitions of the shock and sound together, given at intervals of from 4 to 10 sec. 75 attempts were made to call out the conditioned reaction, each one being preceded on the average by 25 repetitions of the shock and sound together. The very irregular functioning of the conditioned reaction is shown under 'C.R. (sound).' A dash in the table indicates that the subject did not react within 1 sec.

TABLE IV

SUBJECT *B. C.*, FEB. 2, 1:00 P.M.–3:00 P.M.

Sound	R. T.	Shock	Sound and Shock	C. R. (Sound)		Sound and Shock	Shock	R. T.
—	184	118	106	—	362	—	—	229
—	632	112	114	208	199	150	161	—
—	348	120	108	199	—	144	—	289
—	320	117	113	164	190	186	161	211
—	216	118	117	160	245	168	179	236
—	345	120	109	234	199	153	144	222
—	397	118	109	126	214	—	203	232
287	172	112	108	143	169	—	—	232
—	303	108	114	155	172	161	168	251
—	153	108	111	174	—	145	144	171
475	355	108	108	235	172	—	142	187
301	199	108	108	204	188	132	166	230
333	194	112	112	251	282	174	150	216
372	210	110	114	160		169	155	216
783	191	117	111	160		149	150	283
—	206	114	108	95		145	149	305
—	349	111	106	197		—	153	156
—	200	116	112	203		174	130	—

TABLE IV. (*Continued.*)

SUBJECT *B. C.*, FEB. 2, 1:00 P.M.–3:00 P.M.

Sound	R. T.	Shock	Sound and Shock	C. R. (Sound)	Sound and Shock	Shock	R. T.
—	113	108	117	208	173	130	201
—	209	108	110	205	—	139	236
—	218		113	—	—		176
—	253		115	171	170		216
—	215		116	204	152		215
	223		114	—	—		235
	255			240	156		253
				213			206
				40			194
Meas.	25	20	24	35	17	17	25
Av.	258.4 ±14.08	113.1 ±2.08	111.4 ±0.48	192.6 ±6.03	158.9 ±2.27	154.4 ±2.88	223.9 ±4.68
S.D.	104.4	13.8	3.5	52.9	13.9	17.6	34.7
Q_1	199	108	108	164	149	144	206
Q_2	216 ±17.65	112 ±2.61	111.5 ±0.60	199 ±7.56	156 ±2.84	150 ±3.61	222 ±5.87
Q_3	320	117.5	114	213	170	161	236

The conditioning stimulus in the above table was the click of relay, which did not cause reflex winking at the beginning of the experiment. My procedure in measuring the time of winking to the *shock alone* and to the *compound shock-sound stimulus* (columns 3 and 4) was as follows. I would take 1 or 2 measures of the shock time; then 1 or 2 measures of shock-sound time; then 2 or 3 of shock time; then 2 or 3 of the other; and so on, in an irregular manner. The two distributions of measures (columns 3 and 4) were therefore taken under the same conditions, and they may be fairly compared with each other. The same procedure was followed in making the measures shown in columns 7 and 8, so that they may also be fairly compared with each other. During the 'training series' proper, no measures were taken of the time of winking to the compound shock-sound stimulus. While columns 4 and 7 (headed 'Sound and Shock') represent some 'training,' these measures were used mainly in comparisons. When referring to the 'training series,' I always have in mind those compound shock-sound stimuli which immediately preceded the various measures of the conditioned reaction, and whose times were not measured. There were 1000 repetitions of the training stimuli. 40 tests for the conditioned reaction were made, and each was preceded on the average by 25 repetitions of the training stimuli.

reaction time to the sound was *faster* at the end than at the beginning of the experiment. The conditioned reaction was very halting and irregular. The conditioned reaction time, however, considering the last 22 measures, is faster than the reaction time:

	C.R.	R.T.
Av.	182.2 ± 15.66	248.2 ± 5.80
Med.	195.0 ± 19.63	252.0 ± 7.29
R.T.–C.R. (Av.)	+ 66.0 ± 16.70	Reliability = 99.61/100
" (Med.)	+ 57.0 ± 20.94	" = 97.31/100

Table IV. shows the results for subject *B. C.* The reaction time speeds up a little during the experiment. The conditioned reaction functioned from the beginning, and was faster than the voluntary reaction time.

	C.R.	R.T.
Av.	192.6 ± 6.03	223.9 ± 4.68
Med.	199.0 ± 7.56	222.0 ± 5.87
R.T.–C.R. (Av.)	+ 31.30 ± 7.63	Reliability = 99.71/100
" (Med.)	+ 23.00 ± 9.57	" = 94.74/100

A comparison was made in the case of this subject between the speed of winking to the shock alone and to the shock and sound together, both before and after the training series. Before training, the averages are 113σ and 111σ (23). The presence of the sound, after training, seems to slow up the speed of winking a little, though the difference is not reliable. Just the opposite result was obtained in the case of every other subject, where anything like a fair comparison could be made. In all other cases except one this, the presence of the sound seemed to speed up the winking *more* after the training series than before.

Table V. is a summary of the results obtained with subject *D. K.* The conditioned eyelid reaction obtained in this experiment was very unstable, and was quickly lost. The conditioned reaction time, however, was shorter than the voluntary reaction time.

	C.R.	R.T.
Av.	216.9 ± 30.60	288.5 ± 18.89
Med.	151.0 ± 38.35	284.0 ± 23.67
R.T.–C.R. (Av.)	+ 71.6 ± 35.96	Reliability = 91.04/100
" (Med.)	+ 133.0 ± 45.06	" = 97.67/100

TABLE V

SUBJECT *D. K.*, FEB. 5, 4:15 P.M.–6:00 P.M.

	Shock	Sound and Shock	C. R. (Sound)	Sound and Shock (in C. R. Series)	Sound and Shock	Shock	'Unlearning'	R. T.
Meas.	22	19	9 (last)	12	18	10		25
Av.	103.5 ±0.52	102.7 ±0.71	216.9 ±30.60	128.4 ±1.62	124.1 ±1.51	131.3 ±1.56	C. R.	288.5 ±18.89
S.D.	3.6	4.6	136.1	8.2	9.5	7.3	was	140.0
Q_1	100	98	131	125	115	123	quickly	159
Q_2	104.5 ±0.65	105.0 ±0.89	151.0 ±38.35	126.5 ±2.03	124.5 ±1.89	131.5 ±1.96	lost	284.0 ±23.67
Q_3	107	106	359	131.5	132	138		395

In the above table, the conditioning stimulus was the sound of a relay, which did not cause a wink at the beginning of the experiment. The measures of the time of winking to the shock alone and to the compound shock-sound stimulus (columns 1 and 2, also 5 and 6) were taken as described in connection with Table IV. During the training series, occasional measurements of the time of winking to the shock-sound stimulus were made (column 5). Each test for the conditioned reaction was preceded on the average by 25 repetitions of the training stimuli. The conditioned reaction was called out 12 times, out of 20 attempts. This subject was highly sensitive to the shock, and the experiment had to be discontinued sooner than would otherwise have been desired.

In the experiment with this subject, a comparison could be made between the time of winking to the shock stimulus with the sound and without the sound, both before and after training. The *shock and sound* had been given together some 500 times, so it now seemed plausible that the presence of the sound would have some effect on the speed of winking, when the shock was given. This indeed was the case, as the following figures show:

Before Training:	(*a*) Sound and Shock	102.7 ± 0.71
	(*b*) Shock alone	103.5 ± 0.53
	Effect of sound, $a - b$	− 0.8 ± 0.886
	$\frac{a-b}{\frac{a+b}{2}}$	− 0.00776 ± 0.00859
After Training:	(*c*) Sound and Shock	124.1 ± 1.51
	(*d*) Shock alone	131.3 ± 1.56
	Effect of sound, $c - d$	− 7.2 ± 2.171
	$\frac{c-d}{\frac{c+d}{2}}$	− 0.05638 ± 0.01700
Effect of training, $\frac{c-d}{\frac{c+d}{2}} - \frac{a-b}{\frac{a+b}{2}}$,		−0.04862 ± 0.019

Reliability = 95.74/100

The chances are 95 out of 100 that $\frac{c-d}{\frac{c+d}{2}} - \frac{a-b}{\frac{a+b}{2}}$ will be equal to or less than zero. Or, stated in another and perhaps a better way, the chances are 95 out of 100 that, if the experiment is repeated under the same conditions, the sound will have more of a speeding-up effect on the wink time (called out by the shock) *after* the training series than *before*. While this conclusion seems to be correct as stated, it does not logically follow that it may be taken 100 per cent. as evidence of a conditioned reaction. Still it seems difficult to explain the matter any other way. Negative adaptation and fatigue would apparently operate to make the effect of the sound *less after* the training than *before*. It seems that this result has been attained *in spite of the effects of adaptation and fatigue*. I think it may be safely stated that the shock → wink situation has been definitely 'associated' with the sound stimulus, so that it now functions more 'smoothly' when the conditioning (sound) stimulus is present.

One further possibility should not be overlooked. This is the possibility that adaptation to the shock proceeds faster than adaptation to the sound. If such were the case, then the calculations mentioned just above would be considerably vitiated. However, the changes in procedure at different times afford some data on the basis of which the conclusion may be fairly reached that adaptation is faster to the *sound* than to the *shock*. The results of Tables I., II., and III. show clearly that adaptation to the shock (as measured by the speed of winking) is rather small. The results of these three experiments show that the shock continues to call out a wink readily, after a very large number of repetitions. In contrast with this, Tables VIII. and IX. show that the adaptation factor in a reflex wink (to a sound) is fairly rapid. In column 1 of Table VIII., it is to be noted that the sound ceases to call out a wink reflexly after only 26 repetitions. In the 1st 3 columns of Table IX., the reflex (?) winking rapidly becomes very irregular. I made an effort to call out winks reflexly by a very loud sound in the case of 6 subjects. It did not work satisfactorily for more than a dozen or so repetitions! There is no reason at all for *assuming* that adaptation to the shock is greater than adaptation to the sound. As a matter of fact, *the reverse seems to be true*, and the conclusion that "the sound has more of a speeding up effect on the wink time (called out by the shock) after the training series than before" seems to be quite justified.

One possible factor in the 'fatigue' to the shock was the matter of electrical polarization, which was of course present in every experiment (see 24). Although I did not obtain a quantitative measure of the effect of the electrical polarization, its effect (on the speed of winking to the shock) was doubtless less than that of the general fatigue factor. That is to say, the polarization factor was probably less efficacious than the fatigue of the muscles, end plates, nerve cells, and so on.

One objective measure of the total fatigue is shown in the various tables of results in the increased time of winking to the shock.

TABLE VI

SUBJECT *E. B.*, FEB. 7, 10:00 A.M.–1:00 P.M.

	Shock	Sound and Shock	C. R. (Sound)	Sound and Shock (in C. R. Series)	Sound and Shock	Shock	R. T.
Meas.	24	24		10	30	29	38
Av.	118.4 ± 1.20	115.8 ± 0.95	C. R.	128.6 ± 2.07	118.7 ± 0.86	125.6 ± 1.00	218.8 ± 10.57
S.D.	8.7	6.9	was	9.7	7.0	8.0	96.6
Q_1	109.5	110	not	120	112	118	155
Q_2	117.0 ± 1.50	114.5 ± 1.19	secured	126.0 ± 2.59	117.0 ± 1.08	126.0 ± 1.25	199.0 ± 13.25
Q_3	126	122		138	127	128	273

In the above table, the conditioning stimulus was the sound of a relay, which did not cause a wink at the beginning of the experiment. The measures of the time of winking to the shock alone and to the compound shock-sound stimulus were taken as described in connection with Table IV. Each test for the conditioned reaction was preceded by 50 repetitions of the training stimuli. The training stimuli were repeated 1200 times, but no conditioned reaction at all was obtained.

Tables III. and VI. give results which were obtained with the same subject, *E.B.* In the case of Table III., the conditioned reaction was very irregular; and in the present trial (Table VI.) no conditioned reaction at all was established. But in the present experiment some conditioned effect *may* have been present, even though it was not strong enough to call out a wink reaction. Considering shock and sound-shock times, the *presence of the sound* in this case had a speeding-up effect of $2.6\sigma \pm 1.52\sigma$ before training, and a speeding-up effect of $6.9\sigma \pm 1.32\sigma$ after training. The difference between the two, or the *effect of training*, is represented by the figure, 4.3 ± 2.01; and the chances are 93 out of 100 that, if the experiment is repeated under the same conditions, the presence of the sound will speed up the time of winking more *after* the training series than *before*. Several time measures of the sound-shock stimuli were made in the course of the training series; and these figures give a fair indication I think of a lower limit to be expected for the conditioned reaction time.

Table VII. is a summary of the results for subject *E. G.* One peculiarity in this experiment, which is not shown in the table, is the fact that the conditioned reaction functioned slightly better near the beginning of the training series than at the end. It did not function regularly at any time, but in the 13 cases where it did function the time is rather fast.

	C.R.	R.T.
Av.	122.3 ± 16.22	244.5 ± 10.10
Med.	104.0 ± 20.33	222.5 ± 12.66
R.T.–C.R. (Av.)	122.2 ± 19.11	Reliability = approx. 100/100
" (Med.)	118.5 ± 23.95	" = " 100/100

In this case, the average of the conditioned reaction time (122.3σ) came near reaching the time of winking to the sound and shock together in the C.R. series (110.8σ). A comparison of the shock and sound-shock times before and after training shows that the presence of the sound increased the speed of winking (to the shock) $0.4\sigma \pm 1.23\sigma$ before training, and $5.0\sigma \pm 1.94\sigma$ after training. The effect of

TABLE VII

SUBJECT *E. G.*, FEB. 7, 3:30 P.M.-5:30 P.M.

	Shock	Sound and Shock	C. R. (Sound)	Sound and Shock (in C. R. Series)	Sound and Shock	Shock	R. T.
Meas.	28	28	13 (all)	20	24	19	30
Av.	115.6 ± 1.01	115.2 ± 0.71	122.3 ± 16.22	110.8 ± 1.57	118.4 ± 0.99	123.4 ± 1.67	244.5 ± 10.10
S.D	7.9	5.6	86.7	10.4	7.2	10.8	82.0
Q_1	111.5	111	62	98.5	115.5	112	200
Q_2	113.5 ± 1.27	115.0 ± 0.89	104.0 ± 20.33	111.0 ± 1.97	120.5 ± 1.24	125.0 ± 2.09	222.5 ± 12.66
Q_3	122	119	135	118.5	124	135	283

In the above table, the conditioning stimulus was the sound of a relay, which did not cause a wink at the beginning of the experiment. Each of the 31 tests for the conditioned reaction was preceded (on the average) by 25 repetitions of the training stimuli. The training stimuli were repeated 775 times in all. The conditioned reaction functioned only 13 times, out of the 31 trials. The time measure of the conditioned reaction above represents the time for those cases when it did function.

TABLE VIII

SUBJECT *M. W.*, FEB. 7, 8:00 P.M.–10:45 P.M.

Sound (Loud)	Shock	Sound and Shock	C. R. (Sound)	Shock Alone	C. R. (Sound)	Shock Alone	Sound and Shock (in C. R. Series)	Sound and Shock	Shock	'Unlearning'	R. T.
—	99	102	106	104	113	101	102	89	108	230	252
—	98	88	—	90	193	—	103	102	112	—	352
103	95	99	102	97	112	110	106	104	104	200	284
106	89	89	211	109	175	305	91	99	106	223	257
95	89	99	—	148	176	161	88	98	103	511	264
78	90	85	—	—	202	111	89	103	103	305	138
80	99	86	—	110	146	113	88	99	209	349	321
83	101	84	—	115	123	122	97	105	105	478	320
98	99	85	163	147	176	114	102	93	106	278	348
99	98	89	—	—	167	102	106	84	422	194	335
93	97	84	—	78	218	114	102	108	266	209	382
100	102	84	—	85	172	113	94	91	213	98	395
—	97	89	88	92	214	122	98	96	112	196	198
122	96	94	—	95	198	287	100	105	105	245	245
97	103	84	36	94	210	106		105	229	275	221
89	98	86	28	110	168	116		91	106	199	262
106	102	98	—	77				99	212	247	258
104	96	90	111	122				99	362	236	402
99	96	92	144	112				84	96	461	197
100	97	88	174	109				101	355	135	419
100	99	83	143	101				93	106	497	402
95	95	83	105	109				101	103	333	350
103	96	91	171	130				96		542	220

TABLE VIII. (*Continued.*)

SUBJECT *M. W.*, FEB. 7, 8:00 P.M.–10:45 P.M.

Sound (Loud)	Shock	Sound and Shock	C. R. (Sound)	Shock Alone	C. R. (Sound)	Shock Alone	Sound and Shock (in C. R. Series)	Sound and Shock	Shock	'Unlearning'	R. T.
89	99	89	190	109				105		144	316
97	93	93	113	113				97		113	152
125	94	94	126	108				95		—	423
98	98	96	134	110				94		—	
122	94	92	184	109				104		—	
113	98	89	189	99				98		293	
—	99	94	223	104				94		—	
—	103	80	169	106				104		—	
132	97	94	128	102				106		—	
—	95	88	105	103				95		155	
—	94	92	146	103				101		—	
—	97	90	106	101				105		—	
108		92	156	83				105		725	
—		89	155	107						744	
—		95	422	113						520	
475		94	305	120						531	
Meas. . 26 (1st)	35	39			36 (last)	36 (last)	14	36	22	25 (last)	26
Av. ... 99.8 ±1.52	96.9 ±0.39	90.0 ±0.54			172.4 ±6.63	120.8 ±4.96	97.1 ±1.13	98.6 ±0.67	170.1 ±14.08	327.9 ±24.39	296.7 ±10.67
S.D. .. 11.5	3.4	5.0			59.0	44.1	6.3	6.0	97.9	180.8	80.7
Q_1 95	95	86			131	103.5	91	94.5	105	196	245
Q_2 .... 99.0 ±1.90	97 ±0.49	89 ±0.68			170 ±8.32	109.5 ±6.22	99 ±1.42	99 ±0.84	107 ±17.65	275 ±30.51	300 ±13.37
Q_3 104	99	94			191.5	114	102	104	213	478	352

In the above table, the conditioning stimulus was a loud telegraph sounder, which caused winks at the beginning of the experiment as shown in column 1. There were 1,650 repetitions of the training stimuli. The measures of 'C. R. (Sound)' and 'Shock Alone,' in columns 4 and 5 (*continued* in columns 6 and 7) were taken along with each other (the same procedure as described in connection with Table IV. 55 attempts were made to call out the conditioned recation, and it functioned 45 times.

training is given by $4.60\sigma \pm 2.30\sigma$, and the conclusion that the sound speeds up the winking more after the training than before is true with a reliability of 91 cases out of 100.

Table VIII. gives the complete results for subject *M. W.* In this particular experiment I wished to try out the general effect of a sound which at the beginning of the experiment *did* cause a reflex wink. Column 1 shows the effect of this sound in calling out a wink reflexly at the beginning of the experiment. After some 26 repetitions of the sound, it no longer called out a wink reflexly. Measurements of the time of winking to the shock alone and to the shock-sound stimulus were made as described in connection with Table IV. These two distributions have very small variabilities. As a further variation in the procedure, I took measures of the shock time, designated 'shock alone,' during the training series (columns 5 and 7). I also took the usual measures of the time of winking to the compound sound-shock stimulus during the training series (column 8). The sound stimulus not only lost its power of evoking a wink reflexly after 26 repetitions at the beginning of the experiment (column 1 of the table), but also did not call out a wink even after the first 400 or so repetitions of the training stimuli had been given. The sound stimulus did not begin to call out a wink regularly again until about 500 repetitions of the training stimuli had been given. This winking was a *conditioned reaction.* It was secured *in spite of* the auditory adaptation or the fatigue factor plainly evident at the beginning of the experiment. Whatever factor or factors caused the eyelid reflex to *cease* when the sound was repeated at the *beginning* of the experiment were now in all probability *still more* active. But the tendency towards the forming of a connection between winking, caused by the shock, and the sound, was probably *stronger* than the above factor, and had the 'right of way' to such an extent that the conditioned reaction did not fail to function a single time during the last 38 trials! It cannot be claimed that this is a reflex wink caused naturally by the sound, because it has already been demonstrated with

even stronger stimuli that this functions irregularly, and most of the time not at all. The 'unlearning' of this connection is shown in next to the last column of the table.

A comparison between the voluntary reaction time and the conditioned reaction time to the *same* sound shows that the latter is definitely faster.

	C.R.	R.T.
Av.....	172.4 ± 6.63	296.7 ± 10.67
Med.....	170.0 ± 8.32	300.0 ± 13.37
R.T.–C.R. (Av.).....	+ 124.3 ± 12.56	Reliability = approx. 100/100
" (Med.)	+ 130.0 ± 15.75	" = " 100/000

Any factors which tended to make the voluntary reaction time *slow* had exactly the same chance I think of making the conditioned reaction times *slow*. The voluntary factors seem to be definitely ruled out in this case, because of the *speed* of the conditioned reaction.

A comparison between the shock time and the shock-sound time shows that the presence of the sound increased the speed of winking (to the shock stimulus) $6.9\sigma \pm 0.67\sigma$ before training, and $71.5\sigma \pm 14.09\sigma$ after training. The effect of training was $64.6\sigma \pm 14.11\sigma$, and the conclusion that the training had some effect is true with a reliability of 99 out of 100. The measures of *shock-sound* times slow up a little during the experiment. They are 90.0σ, before training; 97.1σ, during training; and 98.6σ, after training. The variability remains very nearly the same: the S.D.'s being 5.0, 6.3, and 6.0. The measures of *shock* alone, on the other hand, slow up much faster. They are 96.9σ, before training; 120.8σ, during training; and 170.1σ, after training. The variability increases rapidly; the S.D'.s being 3.4, 44.1, and 97.9. The conclusion which may be fairly drawn from this unusual behavior is that the sound and the wink were 'connected' with each other, so that immediately after the training series was over the presence of the sound helped the shock to call out a wink very regularly (S.D. = 6.0), whereas the absence of the sound now caused marked irregularities in the distribution (S.D. = 97.9, see third to last

TABLE IX

SUBJECT *M. A.*, FEB. 11, 9:15 A.M.–12:00 M.

Sound (Loud)			Shock		Sound and Shock	C. R. (Sound)	Shock Alone	Sound and Shock (in C.R.Series)	Shock	Sound and Shock	'Unlearning'	R. T.
—	—	305	107	106	95	132			104	143	156	148
—	508	—	104	105	93	144			199	84	117	272
379	309	—	102	115	85	125			105	97	163	450
—	379	440	108	105	88	77			74	106	—	226
349	—	—	107	115	92	104			99	97	205	281
604	296	—	99	103	99	149			102	93	278	460
—	—	199	91	96	102	45			116	99	—	210
—	—	—	97	123	102	253			106	94	255	256
198	419	—	105	129	95	113			107	110	311	277
320	652	324	113	105	84	130			95	106	207	247
222	—	—	100	115	96	132			224	122	143	252
—	764	—	125	118	103	192		99	122	96	225	342
298	225	508	145		103	103		107	257	115	388	232
571	—	—	97		93	260		80	102	103	149	253
—	—	—	136		94	116		85	193	96	226	324
471	—	804	97		102	124	95	97	—	115	112	144
446	265	—	109		96	139	110	106	117	97	514	199
316	305	—	144		97	150	72	99	475	126	144	271
376	442	—	117		106	176	130	113	485	105	134	294
188	382	—	113		95	343	229	109	205	105	162	565
—	—	589	111		92	155	104	108	642	113	188	191
349	403	298	125		85	51	103	103	116	104	240	169
322	359	—	118		104	34	122	94	123	118	205	196
278	—	—	109		106	121	215	102	—	95	183	171
356	—		134		105	189	103	101	783	114	411	—
—	—		111		108	111	125	108	—	125	107	268

TABLE IX. (*Continued.*)

SUBJECT *M. A.*, FEB. 11, 9:15 A.M.–12:00 M.

Sound (Loud)		Shock	Sound and Shock	C. R. (Sound)	Shock Alone	Sound and Shock (in C.R. Series)	Shock	Sound and Shock	'Unlearning'	R. T.
509	—	99	99	154	122	109	114	99	194	155
249	302	109	99	253	108	121	465	131	220	
233	—	108	110	112	102	99	135	124	—	
410	—	122	103	122	110	112	127	127	—	
329	—	132	94	164	244	107	116	113	349	
508	—	129	97			110	97	97	237	
297	—	127	95				116	119	148	
373	319	74	84				194	—	385	
328	—	106	94				246	110	—	
329	—	108	106				196	101	289	
395	—	105	106				107	111	220	
—	—	125	110				138	95	210	
Meas. . .	24 (last)	50	38	31	16	21	35	37	25 (last)	26
Av.	408.2 ±21.38	112.1 ±1.30	97.8 ±0.78	144.3 ±2.47	130.9 ±8.31	103.3 ±1.34	200.1 ±18.76	108.2 ±1.41	231.3 ±15.97	263.6 ±13.03
S.D.	155.3	13.6	7.1	20.4	49.3	9.1	164.5	12.7	118.4	98.5
Q_1	303.5	105	94	112	103	99	106	97	149	196
Q_2	369.0 ±26.8	109 ±1.63	97 ±0.98	132 ±3.1	110 ±10.41	106 ±1.68	117 ±23.51	106 ±1.77	210 ±20.02	252.5 ±16.33
Q_3	475	122	103	164	111	109	205	115	240	281

In the above table, the conditioning stimulus was a telegraph sounder, which called out winks at the beginning of the experiment as shown in the first three columns. Each of the 31 measures of the conditioned reaction time, and also each of the 16 measures of the time of winking to the shock alone (column 8) was preceded on the average by 25 repetitions of the training stimuli. There were 1,175 repetitions of the training stimuli in all.

column)! After a certain point in the training series the conditioned reaction time actually slowed up somewhat. This may or may not have been due to fatigue.

The complete results obtained with subject *M. A.* are shown in Table IX. These results are of the same general nature as those just described in connection with Table VIII.; and the same general trends are again positively indicated. The subject winked quite often to the sound stimulus (which was not as loud as that used for the 'sound reflex' time earlier), at the beginning of the experiment, but the winks were delayed and irregular. The time of a *reflex* wink to sound, which has been discussed in section 2, is exceptionally fast. It seems therefore that the long times, as shown in the first three columns of Table IX., could not properly be called 'reflex' times. Very few of these times are less than 200σ, and a 'reflex' wink to sound is certainly less than 100σ. This same sound was *repeated* some 1,200 times in the course of the experiment; and there is every reason to believe that the slight tendency to wink reflexly to this sound, present at the start, became weaker as the experiment proceeded. Certainly the tendency would not become stronger. A comparison between the voluntary reaction time and the conditioned reaction time shows the former to be *definitely* slower.

	C.R.	R.T.
Av.	144.3 ± 2.47	263.6 ± 13.03
Med.	132.0 ± 3.10	252.5 ± 16.33
R.T.–C.R. (Av.)	$+ 119.3 \pm 13.26$	Reliability = approx. 100/100
" (Med.)	$+ 120.5 \pm 16.62$	" = " 100/100

It is noticeable that the conditioned reaction functioned perfectly from the very beginning of the training series (*i.e.*, there were no 'blanks' in the series), and that the time of the conditioned reaction is *very fast.* The average conditioned reaction time is $144.3 \pm 2.47\sigma$, while the median time is $132.0 \pm 3.10\sigma$. This time is comparable to the speed with which a *trained* subject can react to a sound by raising the finger from a telegraph key, in the usual reaction time experiment. Reacting with the finger in this way is also known

to be faster and more constant than reactions made with other parts of the body.

A comparison between the effect of the sound on the speed of winking to the shock, before and after the training series, shows that the sound had a speeding-up effect before training of $14.3 \pm 1.51\sigma$, and that after training the sound had a speeding-up effect of $91.9 \pm 18.81\sigma$. The effect of training is given by $77.6 \pm 18.87\sigma$; and the chances that this difference is equal to or greater than zero are 99.71 out of 100. But if the medians of the distributions are considered, instead of the averages, very different results will be obtained.

Considering the results of all these experiments together, the conclusion is certain that the conditioned reaction time *may* be made faster than voluntary factors alone can make it. The conditioned reaction was established when the conditioning stimulus (sound) and the fundamental stimulus (shock) were given at the *same* time.

A very simple experiment was arranged to test the effect of giving the conditioning stimulus a short time *after* the wink occurred. A telegraph sounder was connected with the wink apparatus, so that every time the subject (*H. S.*) winked naturally, the sound was given automatically. The subject was allowed to wink naturally for a continuous period of 5 hours. The sound stimulus was given a short time *after* the impulse to wink had been sent each time, in fact when the natural wink was practically completed. The subject surely winked several thousand times during this period. The apparatus was so connected that I could give the same sound stimulus myself at any time, and measure the time of winking, if such occurred. I gave the sound stimulus myself some 40 times during the latter part of this 5-hour period. The sound did *not* cause a wink at any time. No connection was formed between the wink reaction and the sound stimulus which, during the 5-hour 'training period,' had been given after the natural impulse to wink had been sent each time. This experiment furnishes some further evidence I think against the forming of associations in the backward direction.

Very little has been said so far about introspections. Although our most important results do not depend on introspective reports, the introspections which were taken are worthy of consideration. After a conditioned eyelid reaction had been called out, I would occasionally ask the subject about this conditioned wink. Whenever the time of the wink was at all fast, the subject would *insist* that he was not *aware* of the wink until *after* it had occurred. This was the introspection of every subject where the conditioned reaction was established. Voluntary factors apparently were not the 'cause' of the conditioned reactions. I asked several subjects about any 'images' and 'affective elements,' which might have been present, and they were unanimous in affirming that no such were hanging around, at least not *before* the wink occurred. This is offered as evidence against Mme. Dontchef-Dezeuze's interpretation (9) of the conditioned reaction in terms of images and affective elements. I insisted in the case of some of the subjects that they *should* have images and affective elements *with* the conditioned reaction,—but they were unable to get them even then! I think that this may also be taken as evidence that images and affective elements did not play a *conspicuous* rôle in these conditioned reactions.

The other introspective evidence which I wish to mention is as follows. At the beginning of the second experiment with subject *E. B.* (Table VI.), I requested *E. B.* to judge whether the shock coming alone was stronger or weaker than the shock which was present in the compound shock-sound stimulus. The strength of the electric shocks were as a matter of fact exactly the same in both cases, and this subject judged them to be of equal strength at the beginning of the experiment. But after the compound shock-sound stimulus had been repeated together some 1,200 times *E. B.* insisted that the shock coming alone was decidedly *weaker* than the shock which was present in the shock-sound stimulus, and yet only one shock was used throughout this experiment. When the shock stimulus was now given *alone* (see next to last column, Table VI.), this subject would say: "I got the

shock that time all right, but it was much weaker than usual. Something must be the matter!" The shock coming by itself felt 'weak,' 'creepy,' 'strange,' 'lacking in something'; and it always seemed to contain an element of surprise. This introspective report corresponds somewhat with the objective result, *i.e.*, in respect to the slower time of winking to the shock alone, after the training series. All later subjects were questioned on the same matter, and their introspective reports agree very closely with each other. While the shock alone seemed queer to all of these subjects (after the 'training' had been given), the sound alone seemed quite 'natural' at all times. The explanation of this matter is not very clear to me. This is a rather interesting observation, however, especially in view of the fact that the *speed* of winking to the shock alone or to the shock-sound stimulus is very much faster than the voluntary reaction time. These considerations would suggest I think that the conscious aspects in these phenomena were 'results' or 'by-products' of the *process*, as the process moved along.

At the beginning of all of the above experiments, with the exception of one, the sound stimulus was not effective in causing a reflex wink. The sound would have been an effective stimulus, however, even without any conditioned reaction training, if it had been made intense enough. At the beginning of an experiment, the nervous connections between the sound and the winking reaction were already present, but the threshold was too high for the sound to be effective. In those experiments in which a definite conditioned reaction was obtained, this threshold was *lowered*: sound had now been made an effective stimulus.

Section 7

Conclusions

1. The conditioned reaction time, or the time of winking when the sound stimulus is given alone, may be made so much faster than the voluntary reaction time, or the speed with which the subject is able to wink when the same sound

is heard, that voluntary factors in themselves do not explain the result. In the case of one subject (*M. A.*) the average *conditioned* reaction time was 144.3 ±2.47 sigma, and the median conditioned reaction time was 132.0 ± 3.10 sigma. His average *voluntary* reaction time was 263.6 ± 13.03 sigma; whereas the median voluntary reaction time was 252.5 ± 16.33 sigma. Such a result as this could not have been obtained 'by chance.'

Fatigue to the sound stimulus proceeds faster than fatigue to the shock stimulus. The results of this experiment were obtained in spite of the effects of fatigue and negative auditory adaptation.

Any inhibitory effect which may have been present while the subject was reacting voluntarily to the sound (at the end of the experiment), would have also been functioning in a similar manner for the conditioned reaction if voluntary factors had played an important rôle in speeding up the conditioned reaction. Apprehension causes only a very slight increase in the speed of winking, and certainly not enough to account for the results of this experiment.

2. In almost all of the experiments described above there were very marked irregularities in the time of the conditioned (eyelid) reaction. In most subjects the conditioned reaction times had a rather large variability. In a very few subjects it was difficult to establish any conditioned reaction at all. In a few other subjects, however, the conditioned reaction was easily established and it was also fairly regular.

3. The electric shock was kept practically constant for each subject throughout a single experiment. At the beginning of the experiments, all of the subjects judged the shock coming alone to be equal in strength to the shock present in the compound shock-sound stimulus. *After* the training period, *i.e.*, after the compound shock-sound stimulus had been repeated a thousand or so times, the shock coming alone now seemed to the subject decidedly *weaker* than the shock which was present in the shock-sound stimulus. The objective measures of the time of winking to the shock

alone and to the shock-sound stimulus, after training, show that the latter are faster and more regular. The sound has more of a speeding up and regulating effect on the wink (called out by the shock) after the training series than before. This result cannot be accounted for on the assumption that fatigue to the shock proceeded at a much faster rate than fatigue to the sound. There is every evidence that just the reverse is true. These results were obtained in spite of the effects of negative auditory adaptation and fatigue.

4. The results of the experiment indicate some of the conditions which are favorable for securing a conditioned eyelid reaction. The intensity of the sound should be such that it just fails to call out a natural reflex wink at the beginning of the experiment. When the conditioning stimulus (sound) is given *with* the fundamental stimulus (shock) a large number of times, a conditioned reaction will probably be established. Some evidence is found that a conditioned reaction will *not* be established when the sound stimulus is given shortly *after* the shock stimulus. Or, stated more correctly, giving the sound *after* the wink a large number of times does not cause the wink to become conditioned to the sound.

5. The above experiments have the effect of speeding up the subject's voluntary reaction time, so that he is able to voluntarily wink faster at the end than at the beginning of the experiment.

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AN INSTRUMENT FOR MEASURING CERTAIN ASPECTS OF INTELLIGENCE IN RELATION TO GROWTH, PRACTICE, FATIGUE, AND OTHER INFLUENCES

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It seems likely that psychologists will wish to make repeated measurements of the same individuals in such aspects of intelligence as tests of the type of the Army Alpha measure. Growth, practice and fatigue, are three notable cases. It is convenient and time-saving in such work to have an instrument represented in many alternative forms which are nearly equal in difficulty and whose differences in difficulty are rather exactly known.

In an earlier paper[1] I presented provisional facts for a thirty-minute test[2] (which is essentially a harder Alpha, plus four new tests and one of the old Examination A) prepared in fifteen alternative forms. I am now able to state the relative difficulty of these fifteen forms with much greater precision, precision sufficient probably for any purposes to which such tests are likely to be put.

In the earlier paper it was shown that $A = B + 5$, $J = G + 2\frac{1}{2}$ and $J = C - 1$, with high reliability. I have shown elsewhere that when two forms are given after ten minutes' fore-exercise, the median gain from practice is 8. By using the data just mentioned for J, F, G, and C, and the following data for forms given after ten minutes' fore-exercise, we may equate all forms except K.

[1] *Journal of Applied Psychology*, 1920, vol. 4, pp. 283–288.

[2] Part I of the 1919–1923 Series of the Thorndike Intelligence Examination for High-School Graduates.

Order of Forms	Difference	Amount of Difference: Allowing 8 for Practice	Number of Individuals
B C	*C* − *B*	+ 2	86
B G	*G* − *B*	+ 4	126
C E	*E* − *C*	− 3	254
D C	*D* − *C*	+ 2	168
D E	*E* − *D*	− 3½	312
D E	*E* − *D*	− 4	134
D E	*E* − *D*	− 4½	99
E D	*E* − *D*	− 4	72
E H	*H* − *E*	+ 3½	222
E M	*M* − *E*	− 3	86
E M	*M* − *E*	+ 1½	34
F M	*M* − *F*	0	28
J M	*M* − *J*	− 4½	28
F N	*N* − *F*	− 4	167
N F	*N* − *F*	− ½	70
G H	*H* − *G*	− 2	105
G H	*H* − *G*	+ 1	28
H O	*O* − *H*	− 8	68
I M	*M* − *I*	− 4½	320
I M	*M* − *I*	− 9	69
I M	*M* − *I*	− 6	24
M I	*M* − *I*	− 4½	409
M I	*M* − *I*	− 3½	419
J L	*L* − *J*	− 2½	392
L O	*O* − *L*	0	273
L O	*O* − *L*	− 1	286
O L	*O* − *L*	− ½	54

From the above we have, in comparison with the average, − meaning harder and + meaning easier:

A + 2
B − 3
C + 2
D + 3
E − 1
F 0
G − 1½
H + 1½
I + 2½
J + 1
L − 1½
M − 2
N − 2
O − 2

The computations were made as follows:

$C = J + 1$ $F = J - 1$ $G = J - 2\frac{1}{2}$ are given.

$E - C = -3$ $E - J - 1 = -3$ $E = J - 2$

$L - J = -2\frac{1}{2}$ $L = J - 2\frac{1}{2}$

$O - L = -\frac{1}{2}$ $O - J + 2\frac{1}{2} = -\frac{1}{2}$ $O = J - 3$

$N - F = -4$ from 167 cases
$N - F = -\frac{1}{2}$ from 70 cases
So $N - F = -3$ $N - J + 1 = -3$ $N = J - 4$

But since from other scattered data we have $N = J$, we may estimate N as $J - 3$.

$D - C = 2$ $C = J + 1$ $D - J - 1 = 2$ $D = J + 3$
$n = 168$

$E - D = 4$ $E = J - 2$ $J - 2 - D = -4$ $D = J + 2$
$n = 312 + 134 + 99 + 72$

$M - E = -3$	$M - J + 2 = -3$	$M = J - 5$	$n = 86$
$M - E = 1\frac{1}{2}$	$M - J + 2 = 1\frac{1}{2}$	$M = J - \frac{1}{2}$	$n = 34$
$M - F = 0$	$M - J + 1 = 0$	$M = J - 1$	$n = 28$
$M - J = -4\frac{1}{2}$		$M = J - 4\frac{1}{2}$	$n = 28$

So $M = J - 3$
$M - I = -4\frac{1}{2}$ for $n = 729$
$M - I = -3\frac{1}{2}$ for $n = 419$
$M - I = -9$ for $n = 69$
$M - I = -6$ for $n = 24$

So $M - I = -4$
$M = J - 3$
$J - 3 - I = -4\frac{1}{2}$
$I = J + 1\frac{1}{2}$

$H - E = 3\frac{1}{2}$	$H - J + 2 = 3\frac{1}{2}$	$H = J + 1\frac{1}{2}$	$n = 222$
$H - G = -2$	$H - J - 2\frac{1}{2} = -2$	$H = J + \frac{1}{2}$	$n = 105$
$H - G = 1$	$H - J - 2\frac{1}{2} = 1$	$H = J + 3\frac{1}{2}$	$n = 28$
$H - O = -8$	$H - J - 2\frac{1}{2} = 8$	$H = J - 5\frac{1}{2}$	$n = 68$

So $H = J + \frac{1}{2}$

$C - B = 2$	$J + 1 - B = 2$	$B = J - 1$	$n = 86$
$G - B = 4$	$J - 2\frac{1}{2} - B = 4$	$B = J - 6\frac{1}{2}$	$n = 126$

So $B = J - 4$

Replacing J by (Average + 1) in the above, we have A, B, C, etc., in terms of the average, as shown above.

Since the average score for college entrants in these tests after fore-exercise is about 100, the numbers + 2, − 3, + 2, etc., may be considered as percents.

K was equated as $J - 0.3/4$ in an earlier paper on the assumption that the practice effect from a second to a third full trial was 3. If this practice effect is counted as 4, $K = J - 1\frac{3}{4}$. K is thus very near the average. We have called it 0.

Using these values we determine the practice effect from first to second trial when no fore-exercise is given as follows:

Order	Difference	Raw Difference	Difference Corrected for Difficulty of Forms		Number of Cases	
A F.....	*F* − *A*	10	12		160	
A G.....	*G* − *A*	9	12½		113	
A J.....	*J* − *A*	12	13		154	
B F.....	*F* − *B*	15	12		200	
B G.....	*G* − *B*	9½	8		20	
F G.....	*G* − *F*	10	11½		27	
F J.....	*J* − *F*	13½	12½	12	36	144
G J.....	*J* − *G*	15	17½		33	
J L.....	*L* − *J*	11	13½		28	

This practice effect is thus $12\frac{1}{4}$.

Using the same values we determine the practice effect from second to third of three full trials preceded by no fore-exercise as follows:

Order	Difference	Raw Difference	Difference Corrected for Difficulty of Forms		Number of Cases	
G H.....	*H* − *G*	9	6		113	
F K.....	*K* − *F*	2½	2½		351	
J K.....	*K* − *J*	3	4		144	
H L.....	*L* − *H*	− 1	2		36	
J M.....	*N* − *J*	2½	5½	3½	40	102
G J.....	*J* − *G*	5	½		26	

The practice effect from a second to a third trial thus

seems to be about $3\frac{1}{2}$, but is less reliably determined than the $12\frac{1}{4}$.

Using $12\frac{1}{4}$ and $3\frac{1}{2}$, we may check the determinations of the relative difficulty of the forms from the cases where no fore-exercise was given.

Thus $J - A = 12 - 12\frac{1}{4}$ or $-\frac{1}{4}$ for $n = 154$. We had $J - A = -1$, a difference of $\frac{3}{4}$. $F - A = 10 - 12\frac{1}{4}$ or $-2\frac{1}{4}$ for $n = 160$. We had $F - A = -2$, a difference of $\frac{1}{4}$. $F - B = 15 - 12\frac{1}{4}$ or $2\frac{3}{4}$ for $n = 200$. We had $F - B = 3$, a difference of $\frac{1}{4}$. $G - A = 9 - 12\frac{1}{4}$ or $-3\frac{1}{4}$ for $n = 113$. We had $-3\frac{1}{2}$, a difference of $\frac{1}{4}$. $K - F = 2\frac{1}{2} - 3\frac{1}{2}$ or -1 for $n = 351$. We had $K - F = 0$, a difference of 1. $K - J = 3 - 3\frac{1}{2}$ or $-\frac{1}{2}$ for $n = 144$. We had $K - J = -1$, a difference of $\frac{1}{2}$. The average discrepancy between these differential determinations by the two methods is thus $\frac{1}{2}$.[1] The determinations are thus sufficiently reliable for any use that is likely to be made of the tests, since the median difference between two trials by the same individual is about fifteen times the probable error of the estimate of difficulty.

[1] I have checked further with recently obtained data from tests given after fore-exercise as follows:

The order being	Practice	*I L*,	the median	$L - I = 8$,	$n = 44$
"	"	"	"	*I M*,	"	"	$M - I = 6$,	$n = 40$
"	"	"	"	*I N*,	"	"	$N - I = 5.5$,	$n = 41$
"	"	"	"	*J L*,	"	"	$L - J = 8$,	$n = 41$
"	"	"	"	*J M*,	"	"	$M - J = 7.5$,	$n = 41$
"	"	"	"	*J N*,	"	"	$N - J = 8.5$,	$n = 45$

Equating *I* with *J* by their differences from *L M* and *N* practice we have $I - J = 1.8$ By our standard values $I - J$ was 1.5.

The order being	Practice	*J E*,	the median	$E - J$ was	5.5,	$n = 73$
"	"	"	"	*J I*,	"	"	$I - J$ "	8,	$n = 64$
"	"	"	"	*J L*,	"	"	$L - J$ "	5,	$n = 63$
"	"	"	"	*J O*,	"	"	$O - J$ "	3.5,	$n = 56$

Using the standard values for *E*, *I*, *L* and *O*, and reversing the 8 allowance for practice, we have $J = +2$. By our standard values $J = +1$.

The order being	Practice	*E H*,	the median	$H - E$ was	8.5,	$n = 32$
"	"	"	"	*E M*,	"	"	$M - E$ "	9.5,	$n = 34$
"	"	"	"	*E O*,	"	"	$O - E$ "	6,	$n = 33$

Using the standard values for *H*, *M* and *O*, and reversing the 8 allowance for practice, $E = -0.8$. By our standard it was -1. The discrepancies in these cases are about the same as before.

These differences in difficulty are of course from medians and will fit fairly well the sort of person who scores about 100 in the test. It cannot be in any way guaranteed that, say, Form *B* is 3 per cent. harder than the average form for very low scorers and for very high scorers. I have data from which the differences in difficulty between forms could be computer, for high scorers and low scorers separately, but the matter does not at present seem important enough to justify the great expense of time required.

RELIABILITY OF SCORES IN STEADINESS TESTS

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If significant results are to be obtained from steadiness tests a sufficiently large number of cases must be examined with enough accuracy to validate the conclusions. If too fine measurements are used the work on each individual is unnecessarily increased, the total amount of work done is very great, and the number of cases which one experimenter can examine becomes correspondingly small. However, if the method is simplified too far serious errors occur because of different interpretations which may be drawn from the same data. When a steadiness plate is used the touches are usually recorded either by the kymograph or by an observer with a telephone receiver. In both cases the number of holes used is arbitrarily limited.

When a telephone receiver is used in counting the touches the experiment can be performed more quickly and it need not be done in the laboratory. On the other hand, the difficulties inherent in the recording of light electrical contacts by mechanical means appear to invalidate conclusions drawn from kymographic records. For example; if the plate and stylet are connected in a power circuit (110 volts), with resistance small enough to be certain of the passage of the current when a light contact is made, the arc interferes with the steadiness of the subject and in a short time changes the size of the hole. But if the amperage is low enough to eliminate the arc there is danger that the current which passes through in case of a very light touch will be insufficient to operate the marker which is necessarily wound with low resistance.

If we assume that a record of touches made by a subject in all eleven holes of a plate is a fair index of his steadiness,

but that such a record can not be practicably obtained, several questions arise.

1. Do the obvious disadvantages of the telephone receiver (the difficulty in identifying double touches, the failure to record the duration of contacts, and the introduction of the 'personal equation' as a source of error) outweigh its advantages?

2. If so, is this true only for the smaller holes where a great many touches are made?

3. If the receiver is satisfactory to certain minimal limit of hole-size do the touches made before this limit is reached give a fair index of the subject's steadiness?

4. Or is the variability of the telephonic record independent of the number of touches per hole?

5. If so, is this variability ever negligible?

6. What method of scoring is least affected by these disturbing factors?

The method adopted for the solution of these problems which seemed to be the simplest, was to obtain and compare the records of several subjects' steadiness as recorded by the kymograph and counted simultaneously by two independent observers. In order that no touches could be recorded by the kymograph which were not heard by the observers, one switch threw the motor, marker, and telephone receivers into the circuit at the same time. The Johns Hopkins steadiness plate was used (1). Ten subjects; four undergraduates, four graduate students, and two instructors in psychology, were tested. Schachne Isaacs, instructor in the department of Psychology, and Mildred E. Day, graduate student, acted as observers. Results of the test on ten reactors are given in Tables I. and II.

Summary of Results

Total number of touches recorded by Kymograph	4846
" " " " " Observer 1	4656
" " " " " Observer 2	3872

In Table III. are the rankings of the subjects by the kymograph and two observers based on the total number of touches made by each subject in all of the eleven holes.

TABLE I

TABLE OF RECORDS

Right Hand

Subject	Hole Number											
	1	2	3	4	5	6	7	8	9	10	11	Recorded by
A.....	1	0	1	4	6	7	15	25	31	47	75	Kym
	1	0	1	4	6	6	14	26	34	41	58	0–1
	1	0	1	4	6	5	13	21	24	26	46	0–2
B.....	0	0	0	0	2	4	9	25	24	43	52	Kym
	0	0	0	0	2	3	9	31	32	49	61	0–1
	0	0	0	0	2	3	9	20	22	38	41	0–2
C.....	0	0	0	0	4	4	13	19	27	42	63	Kym
	0	0	0	0	5	4	13	21	28	44	62	0–1
	0	0	0	0	3	4	13	17	24	32	45	0–2
D.....	8	18	9	12	18	37	56	79	102	110	108	Kym
	7	18	10	10	20	33	61	72	89	85	105	0–1
	6	13	10	8	17	24	30	42	51	50	70	0–2
E.....	0	4	2	1	1	12	15	22	29	50	60	Kym
	0	4	3	1	1	11	18	23	33	38	65	0–1
	0	4	2	1	2	12	16	20	29	44	52	0–2
F.....	0	0	0	1	0	1	6	19	15	29	37	Kym
	0	0	0	1	0	1	5	16	13	28	30	0–1
	0	0	0	1	0	1	6	16	12	21	33	0–2
G.....	2	1	2	8	7	11	24	36	45	45	61	Kym
	2	1	2	6	8	10	22	34	37	47	64	0–1
	2	1	2	6	7	10	22	36	40	38	64	0–2
H.....	1	2	0	1	3	1	7	7	29	45	38	Kym
	0	1	0	0	1	1	5	6	34	41	36	0–1
	0	1	0	0	1	1	6	8	35	33	28	0–2
I.....	3	3	0	15	9	7	18	24	36	42	58	Kym
	2	3	0	11	8	7	18	25	30	31	49	0–1
	2	3	1	10	8	7	17	22	28	36	62	0–2
J.....	6	0	0	1	1	2	18	9	20	29	67	Kym
	3	0	0	1	1	2	17	10	19	27	51	0–1
	3	0	0	1	1	2	14	8	20	27	38	0–2

Legend: Kym indicates Kymographic record
0–1 " Observer 1's "
0–2 " " 2's "

The subjects may be ranked on the basis of the last hole previous to the one in which an arbitrarily selected number of touches were made. Table IV. contains a ranking of this type, taking fifteen touches in twenty seconds as a limit. This is the method used by Woolley and Fischer (2), except that they chose twelve touches in fifteen seconds as a limit rather than fifteen touches in twenty seconds. Whipple (3) also suggests this method. Dewey, Child, and Ruml (4) report the test as given in the same way, but in scoring the

separate values for hole and contacts were combined. This makes possible a distribution which can be easily correlated with others but involves the danger of weighting the holes more or less than their difference in size justifies.

Table II

Table of Records

Left Hand

Subject	Hole Number											
	1	2	3	4	5	6	7	8	9	10	11	Recorded by
A.....	1	4	5	1	9	10	30	27	37	43	78	Kym
	1	4	5	1	9	11	30	32	37	43	64	0–1
	1	4	5	1	9	10	24	20	25	35	70	0–2
B.....	0	0	1	2	4	8	28	29	34	57	74	Kym
	0	0	1	2	3	8	30	35	40	62	85	0–1
	0	0	1	2	4	8	25	24	28	44	70	0–2
C.....	3	1	0	2	8	14	14	35	36	43	66	Kym
	1	1	1	1	7	11	13	36	41	40	70	0–1
	1	1	1	1	6	10	10	26	31	37	52	0–2
D.....	3	7	2	4	10	8	25	60	58	58	79	Kym
	2	8	2	5	9	9	38	58	68	78	110	0–1
	2	7	2	4	8	9	17	32	38	44	56	0–2
E.....	16	22	21	41	43	40	62	55	57	66	63	Kym
	12	26	27	39	38	39	50	61	45	48	58	0–1
	12	19	21	29	29	28	45	47	44	58	64	0–2
F.....	0	1	3	1	3	5	17	21	34	40	57	Kym
	0	1	2	1	4	4	15	19	31	35	45	0–1
	0	2	2	1	4	5	15	18	28	37	42	0–2
G.....	6	8	7	9	13	32	42	41	45	55	57	Kym
	5	7	6	8	12	28	33	33	41	51	68	0–1
	6	8	6	8	13	23	40	45	48	52	60	0–2
H.....	3	2	1	12	16	9	22	32	25	36	57	Kym
	2	1	1	8	11	7	16	32	27	38	45	0–1
	2	1	1	7	9	8	17	29	27	20	42	0–2
I.....	6	0	1	4	4	10	10	31	29	46	49	Kym
	6	1	1	2	4	10	9	28	29	36	48	0–1
	6	1	1	3	4	7	9	22	22	46	44	0–2
J.....	2	3	3	8	7	22	24	33	49	52	90	Kym
	2	2	4	4	7	21	23	35	39	42	59	0–1
	2	3	4	5	7	18	22	26	36	36	57	0–2

Legend: Kym indicates Kymographic record
0–1 " Observer 1's "
0–2 " " 2's "

Johnson (5) modified the Cincinnati method so that the cases whose limiting hole was the same were differentiated by their total number of touches rather than by the number made in the last hole. Table IV. contains a ranking on this basis.

TABLE III

RANKING ON BASIS OF TOTAL NUMBER OF CONTACTS MADE IN ALL ELEVEN HOLES OF THE PLATE

	Ranking by Kymograph	Ranking by Observer 1	Ranking by Observer 2
Right Hand.....	*F* 108	*F* 94	*F* 90
	H 134	*H* 125	*H* 113
	J 153	*J* 131	*J* 114
	B 159	*C* 177	*B* 135
	C 172	*I* 184	*C* 138
	E 196	*B* 187	*A* 147
	A 212	*A* 191	*E* 182
	I 215	*E* 220	*I* 196
	G 241	*G* 233	*G* 228
	D 557	*D* 510	*D* 321
Left Hand.....	*F* 182	*F* 157	*F* 154
	I 190	*I* 174	*H* 163
	H 215	*H* 188	*I* 165
	C 222	*C* 222	*C* 176
	B 237	*A* 237	*A* 204
	A 245	*J* 238	*B* 206
	J 293	*B* 266	*J* 216
	D 314	*G* 292	*D* 219
	G 315	*D* 387	*G* 309
	E 486	*E* 443	*E* 396

Legend: Every subject is represented by a letter. The score following each is the total number of his contacts in the eleven holes.

The number of touches in a selected group of holes may be used as a score. With our subjects the sum of the contacts in holes six, seven, and eight gave a very high correlation with the scores based on the total number of touches in all eleven holes, as shown in Table IV. However, with other groups of holes this was not true. Hence this method would hardly be satisfactory for general use since a group of subjects with a different range of steadiness, such as young children, would have to be tested in an entirely different set of holes.

A combination of the above methods might eliminate some of the disadvantages of each. If a maximum total number of contacts, such as fifteen, is allowed and the rating based on the last hole before this limit was reached, one obtains a ranking which is almost identical with that based on the total number of contacts in all eleven holes. An additional advantage is that the records of an observer using a telephone

TABLE IV

	I	II	III	IV
Right Hand ..	*F*	*H* 8-7	*H* 15	*H* 8-22
	H	*F* 7-6	*F* 26	*F* 7-8
	J	*B* 7-9	*J* 29	*B* 7-15
	B	*C* 7-13	*C* 36	*C* 7-21
	C	*J* 6-2	*B* 38	*E* 7-33
	E	*A* 6-7	*A* 47	*A* 7-34
	A	*G* 6-11	*E* 49	*J* 6-10
	I	*E* 6-12	*I* 49	*G* 6-31
	G	*I* 3-0	*G* 71	*I* 6-37
	D	*D* 1-8	*D* 172	*D* 4-8
Left Hand ...	*F*	*I* 7-10	*F* 43	*I* 7-35
	I	*C* 7-14	*I* 51	*C* 7-42
	H	*F* 6-5	*H* 63	*F* 6-13
	C	*B* 6-8	*C* 63	*B* 6-15
	B	*D* 6-8	*B* 65	*A* 6-30
	A	*A* 6-10	*A* 67	*D* 6-34
	J	*J* 5-7	*J* 79	*J* 5-23
	D	*G* 5-13	*D* 93	*G* 5-43
	G	*H* 4-12	*G* 115	*H* 4-18
	E	*E* 0 (1-16)	*E* 157	*E* 0 (1-16)

Legend:

Column I—Ranking based on total number of touches in all eleven holes as recorded by kymograph.

Column II—Rank and score by the method of Wooley and Fischer except that 15 touches in 20 seconds were selected as a limit.

Example: *H* 8-7

H subject

8 hole reached

7 number of contacts in hole 8.

Column III—Ranking and score based on the total number of touches made in holes six, seven, and eight.

Column IV—Ranking by Johnson's method.

Example: *H* 8-22

H subject

8 hole reached (the last in which not more than 15 touches were made)

22 the total number of contacts up to and including hole 8.

receiver will be practically the same as the kymographic record because of the fewness of contacts which must be counted in any one hole.

Example of this method:

Subject *A* made one contact in the first hole, four in the second, five in the third, one in the fourth, and nine in the

fifth. When he has finished with the fourth hole his total is eleven; in the fifth, twenty. Here he has passed his limit of fifteen so his score is 4–11 (4 representing the hole and 11 the number of his contacts).

TABLE V

RANKINGS WHEN A TOTAL OF FIFTEEN CONTACTS IS SELECTED AS A LIMIT COMPARED WITH THE RANKING BASED ON THE TOTAL NUMBER OF CONTACTS IN ALL ELEVEN HOLES

	Kymograph Ranking Based on Total Contacts in All Holes	Rankings on Basis of Fifteen Total Contacts by		
		Kymograph	Observer 1	Observer 2
Right Hand . .	*F*	*F* 7–8	*H* 8–14	*F* 7–8
	H	*B* 6–6	*F* 7–7	*H* 7–9
	J	*H* 6–8	*B* 7–14	*B* 7–14
	B	*C* 6–8	*J* 6–7	*J* 6–7
	C	*J* 6–10	*C* 6–9	*C* 6–7
	E	*E* 5–8	*E* 5–9	*E* 5–9
	A	*A* 5–12	*A* 5–12	*A* 5–12
	I	*G* 4–13	*G* 4–11	*G* 4–11
	G	*I* 3–6	*I* 3–5	*I* 3–6
	D	*D* 1–8	*D* 1–7	*D* 1–6
Left Hand. . .	*F*	*F* 6–13	*F* 6–12	*F* 6–14
	I	*B* 5–7	*B* 5–7	*B* 5–7
	H	*C* 5–14	*C* 5–11	*C* 5–10
	C	*I* 4–11	*I* 5–14	*I* 4–11
	B	*A* 4–11	*A* 4–11	*A* 4–11
	A	*H* 3–6	*H* 4–12	*H* 4–11
	J	*J* 3–8	*J* 4–12	*J* 3–9
	D	*D* 3–12	*D* 3–12	*D* 3–11
	G	*G* 2–14	*G* 2–12	*G* 2–14
	E	*E* 0 (1–16)	*E* 1–12	*E* 1–12

Example: *F* 7–8
F subject
7 hole
8 contacts made up to and including hole 7.

Table V. contains rankings by the kymograph and observers on this basis compared with rankings based on the total contacts made in eleven holes.

Figures 1 and 2 show typical variations of the observers from the kymographic records. Figure 3 contains extracts from several kymographic records indicating that the variations of the observers may depend on the frequency per second

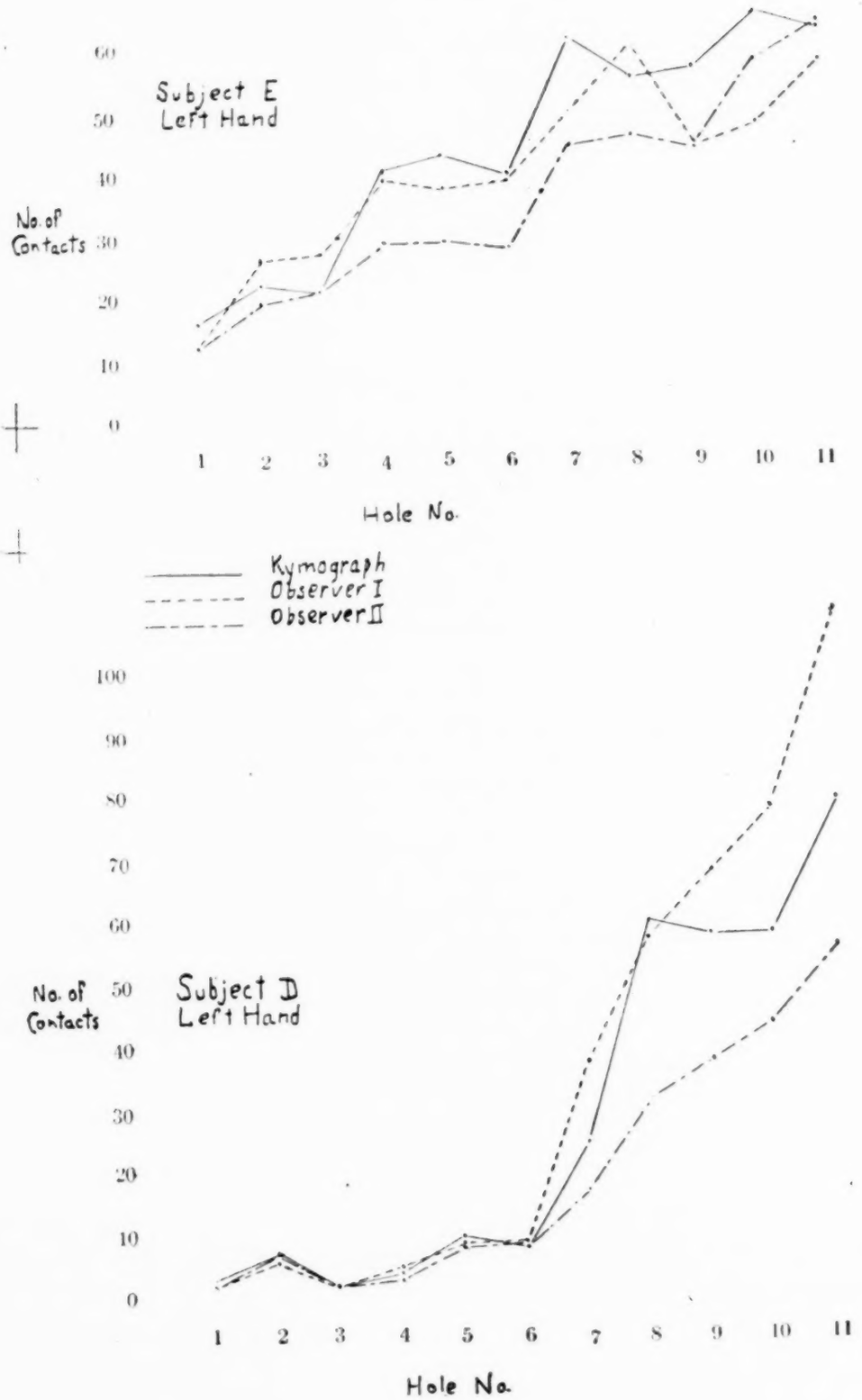

Subject E
Left Hand
No. of Contacts
0
10
20
30
40
50
60
1 2 3 4 5 6 7 8 9 10 11
Hole No.
Kymograph
Observer I
Observer II
Subject D
Left Hand
No. of Contacts
0
10
20
30
40
50
60
70
80
90
100
1 2 3 4 5 6 7 8 9 10 11
Hole No.

FIG. 1.

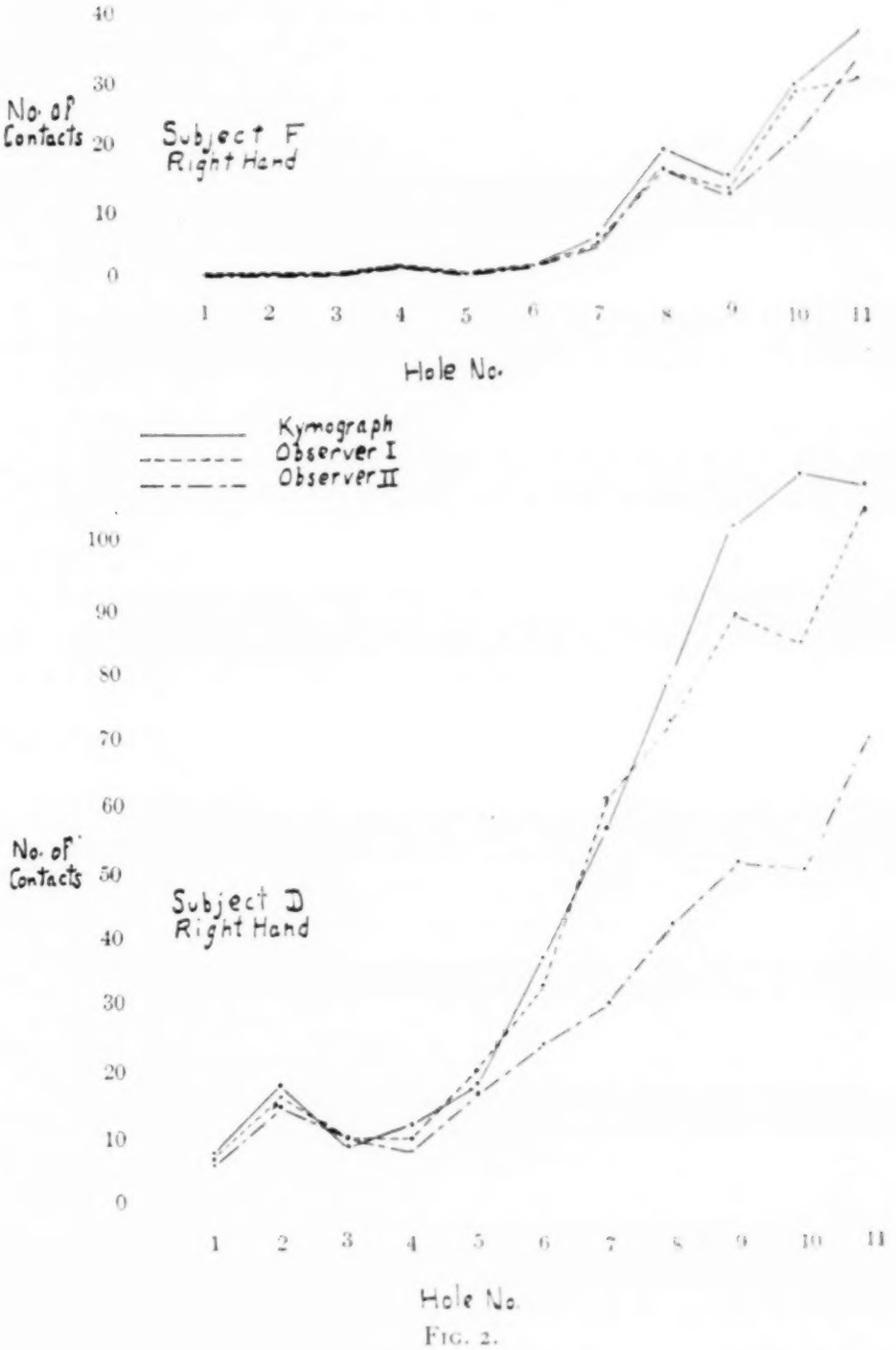
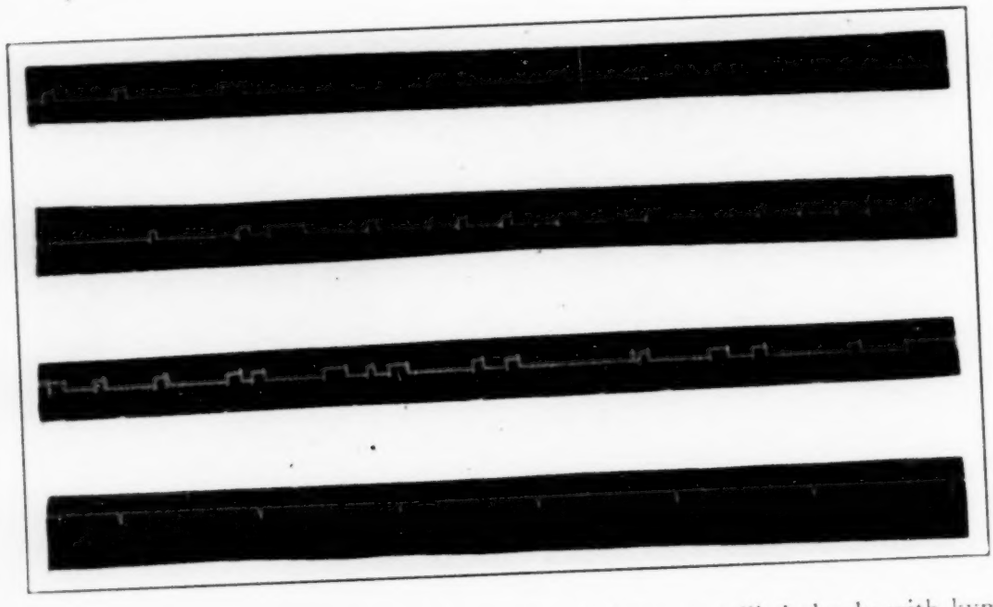


FIG. 2.

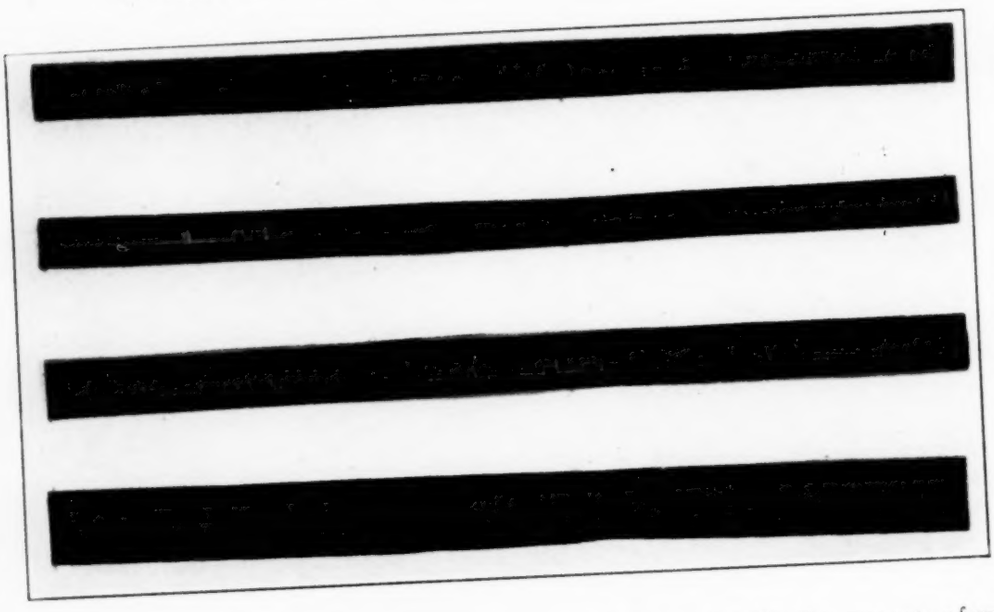
of the contacts rather than on the number made per hole. From this data several inferences might be drawn.

1. The variations of the observers from the kymograph are not uniform. This is obvious from the curves shown in Figs. 1 and 2 which were not selected as examples of especially wide variability.

2. There is a direct relation between the number of touches made in any hole and the reliability of the observers at that point.



Examples of cases in which results of observers tallied closely with kymograph.



The total number of touches is practically identical in the two sets of records.

Capital letter indicates the subject, right or left, the hand used, and the number is that of the hole in which the contacts were made.

A comparison of these records with the table of data will show the actual variation in each case.

FIG. 3. Typical kymographic records showing frequency of contacts in cases where results of observers varied widely

3. But there is no single number of touches that can be arbitrarily given as the limit of the observer because different observers vary in accuracy and the same observer varies with different subjects, as shown in Figs. 1 and 2.

4. Since different methods of scoring based on the same data give different rankings some constant method should be used.

5. Although the data is insufficient for drawing any conclusions relative to a satisfactory method the indications are that:

No single hole or arbitrarily selected group of holes offers a fair index of steadiness.

The method of scoring shown last in the summary of data probably simplifies the test without interfering seriously with its accuracy.

If this method is used the advantages of the telephone receiver do appear to outweigh its disadvantages.

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THE RELIABILITY OF THE SEASHORE PHONOGRAPH RECORD FOR THE MEASUREMENT OF PITCH DISCRIMINATION

BY A. R. GILLILAND AND C. R. JENSEN

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This experiment is in no way concerned with the value of the measurement of pitch discrimination as a factor in the investigation of musical talent. It was conducted with a sympathetic appreciation of such measurements and is merely a criticism of the phonograph as a means of obtaining the records.

While giving the Seashore tests for pitch, intensity, time, consonance, and for tonal memory as arranged for the Columbia graphophone records certain distracting factors seemed to vitiate the results. One of these distracting factors was the scratch of the needle on the phonograph record, another was the always more or less noticeable metallic overtones common to all tones reproduced on the phonograph. It seems reasonable to think that these two factors might seriously affect the student's judgment of pitch. In order to overcome these difficulties it was necessary to construct a suitable apparatus for the production of relatively pure tones of the same pitch as those used by Professor Seashore. This made it possible to directly compare the records obtained by each method.

The Apparatus[1]

The method used for producing relatively pure tones is not so essentially different from that used by Seashore in his experiment described in Psychological Monograph No. 53. The accompanying photograph gives an idea of the apparatus used in this experiment. It consists of eight tuning forks, a standard of 435 v.s. and seven others varying by increments

[1] Much credit is due Mr. E. C. Robes, of the Dartmouth workshop, for valuable suggestions and help in the construction of the apparatus.

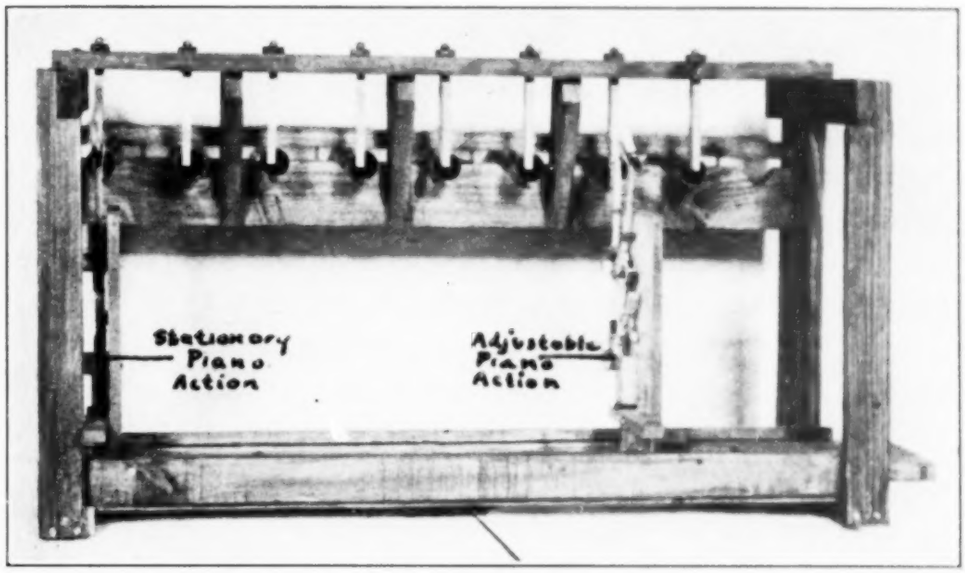


FIG. 1.

of $\frac{1}{2}$, 1, 2, 3, 5, 8, and 12 v.d. respectively. These forks are mounted with resonators made of closed brass cylinders. The forks are struck by piano hammers actuated by ordinary upright piano actions. One piano action was attached to a stationary board for striking the standard fork. Another action was attached to a sliding board in such a way that the hammer could be made to strike any one of the seven other forks. The piano actions and the resonators were very carefully adjusted so that with ordinary precaution in striking the keys the tones produced would vary imperceptibly in intensity. Professor Seashore objects to mounting the forks, saying that they then tend to assume individual peculiarities. The authors encountered the same difficulties at first, but these difficulties were surmounted by the methods described above, together with a very exact sidewise adjustment of the movable hammer. By these means noticeable differences in intensity and overtones were almost entirely eliminated. Seashore's further argument that slight intensity differences are desirable in order to minimize any deliberate or subconscious attempt to discriminate the tones on the basis of intensity differences does not seem especially convincing.

The Method

The subjects were 43 unselected members of a class in elementary psychology. Most of them had taken the Seashore tests some months before and so were familiar with the method. The subjects were given no indication of the purpose of the experiment and they did not know that a comparison of the records was to be made, or even that the same tones were to be used with the apparatus as with the phonograph. The observers were seated in the center of a recitation room 30 feet x 40 feet, their backs to the apparatus, so that they could see none of the procedure. The phonograph test was given first. Since only seven comparisons were to be made with the forks—the seven corresponding to the last seven sets of comparisons on the graphophone—the time during which the first three sets of tones were being run off was used to explain and illustrate how the comparisons were to be made. The directions for recording were given as found in the 'Manual of Instructions for Measures of Musical Talent.' At the beginning of the fourth series on the graphophone, the increment of 12 as given in the key, the recording was started and continued for the rest of the record. Immediately following this the tones were given with the fork apparatus again starting with the increment of 12 and following the key exactly for the tones given by the phonograph record. As has been said the observers did not know that the same order was being followed in the two experiments, furthermore the first records were not in sight while the second experiment was being conducted. The whole time taken for the two experiments was about 20 minutes.

It will be remembered that the three practice series were given first and that the experiment had already been taken once before. Any improvement in results, therefore, could not be attributed to practice. On the other hand because of the short time taken no objection of practical significance could be raised that the results of the second part would be affected by fatigue.

Results

The results are given in the accompanying table. The judgments of each observer as made from the phonograph and as made from the forks are placed side by side. These judgments are calculated according to the per cent. of correct responses. Since only seven series instead of ten were taken with each method these results cannot be directly compared with the table given by Professor Seashore for the relative standing on the basis of one hundred unselected individuals. They are valuable only for the comparisons made in this case. It will be noted that the average phonograph judgments are 75.1 per cent right and that of the forks are 83.0 per cent right, a difference of 7.9 per cent in favor of the forks.

The graph offers another interpretation of the results and probably the more significant one. Not only does it show the difference in the total number of errors but also their distribution. The errors made with the forks are concentrated at the side representing the smallest vibration differences. The total number of errors made with the graphophone was 757 as compared with 510 for the forks; yet the number of errors for the $\frac{1}{2}$ increment was 236 on the forks as compared with 207 for the graphophone. The dotted line on the graph representing the fork errors gradually drops off showing the least errors at 12 as should be expected, whereas the solid line representing the phonograph errors comes to its lowest point at 8 and rises slightly at 12. It will, therefore, be noted from the two curves that the one representing the results for the forks is the much more ideal curve.

The Apparatus Compared with Unmounted Forks

Professor Seashore states that he has discontinued the use of all striking devices and uses only an ordinary hammer with or without a resonator. In order to compare the effects of a relatively constant intensity and quality of tones as produced by the apparatus already described in this article, with relatively pure tones but of varying intensity as obtained by striking forks in the usual way, a further test was made. 30 subjects were used in this part of the experiment.

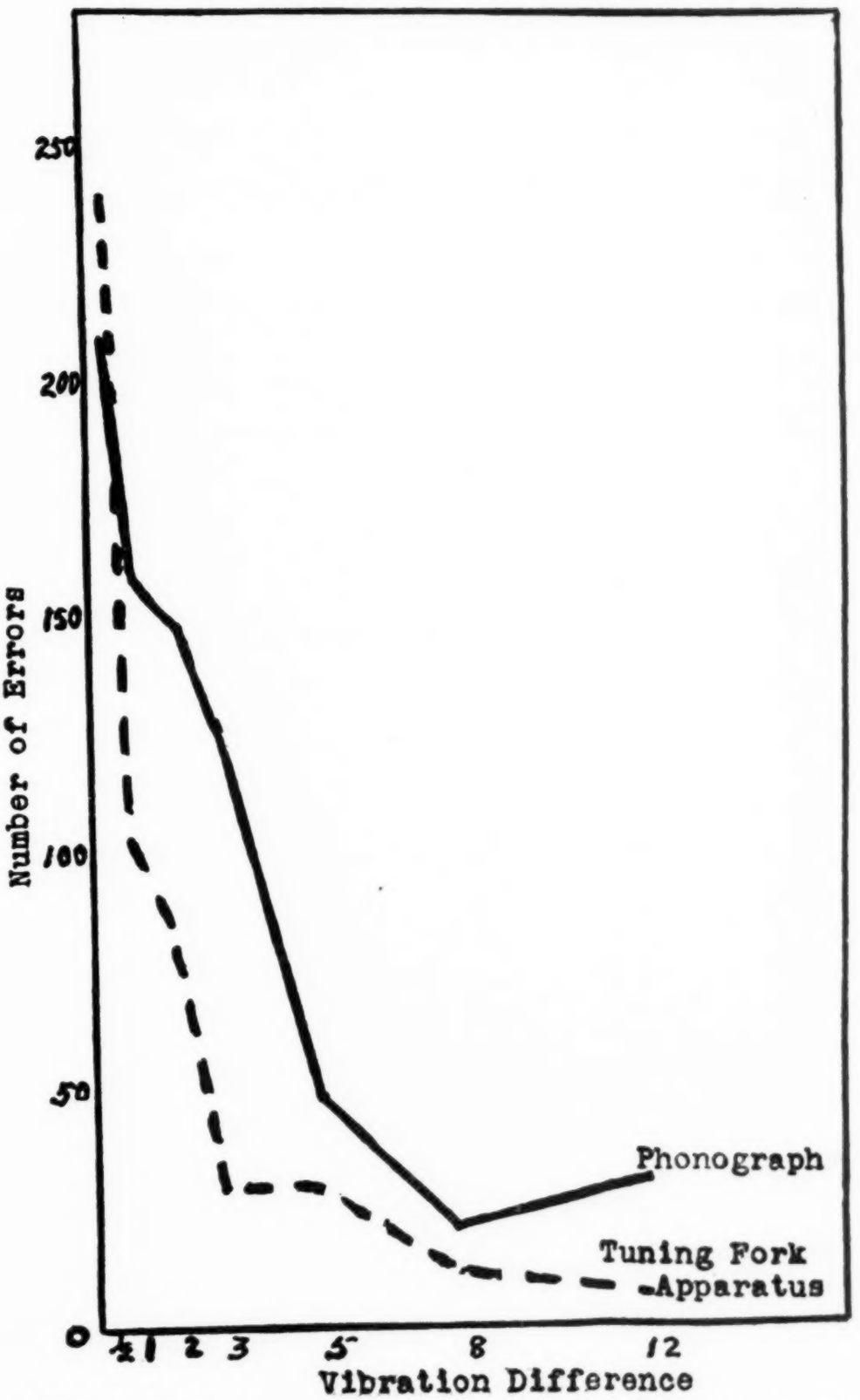
TABLE I

SHOWING PER CENT OF CORRECT RESPONSES OUT OF 70 JUDGMENTS EACH BY 43 COLLEGE STUDENTS (1) WHEN THE SEASHORE RECORD FOR PITCH WAS USED AS THE SOURCE OF THE TONES, AND (2) WHEN MOUNTED TUNING FORKS WERE USED

No.	Phonograph	Forks	Advantage Forks	No.	Phonograph	Forks	Advantage Forks
1	80	84	4	23	61	83	22
2	70	81	11	24	84	89	5
3	73	86	13	25	86	87	1
4	86	83	− 3	26	69	83	14
5	77	77	0	27	77	81	4
6	76	77	1	28	86	86	0
7	84	99	15	29	71	97	26
8	61	80	19	30	83	86	3
9	69	79	10	31	74	91	17
10	89	83	− 6	32	81	96	15
11	66	90	24	33	64	54	−10
12	86	64	−22	34	74	94	20
13	70	90	20	35	79	80	1
14	70	89	19	36	83	86	3
15	83	73	−10	37	70	93	23
16	77	86	9	38	81	81	0
17	74	80	6	39	77	87	10
18	44	80	36	40	81	91	10
19	60	50	−10	41	70	70	0
20	79	87	8	42	74	91	17
21	77	80	3	43	69	86	17
22	84	70	−14				
		Average			75.1	83.0	7.9

METHOD

From a second set of forks the eight forks corresponding to those set up in the apparatus were selected, that is, a standard of 435 v.s. and seven others differing from the standard by increments of $\frac{1}{2}$, 1, 2, 3, 5, 8, and 12 v.d. In general the same method was used as in the first experiment. The subjects were asked to make the same kind of judgments, that is, whether the last of each pair of tones sounded was higher or lower than the first. The same order of sounding the tones was used as before. Each of the seven forks was, therefore, compared ten times with the standard, making seventy judgments in all with the apparatus and the same number with the unmounted forks. In this experiment the unmounted forks were struck upon the knee of the experimenter and then immediately placed in the socket of the



GRAPH I. Showing the number of errors and their relation to the vibration difference of the tones. 43 subjects.

small common resonance box as furnished with the forks by the C. H. Stoelting Co. Following this the same differences were sounded with the apparatus and the subjects were asked to make seventy more judgments. The subjects did not know that the same differences were being used and could not see their previous records.

TABLE II

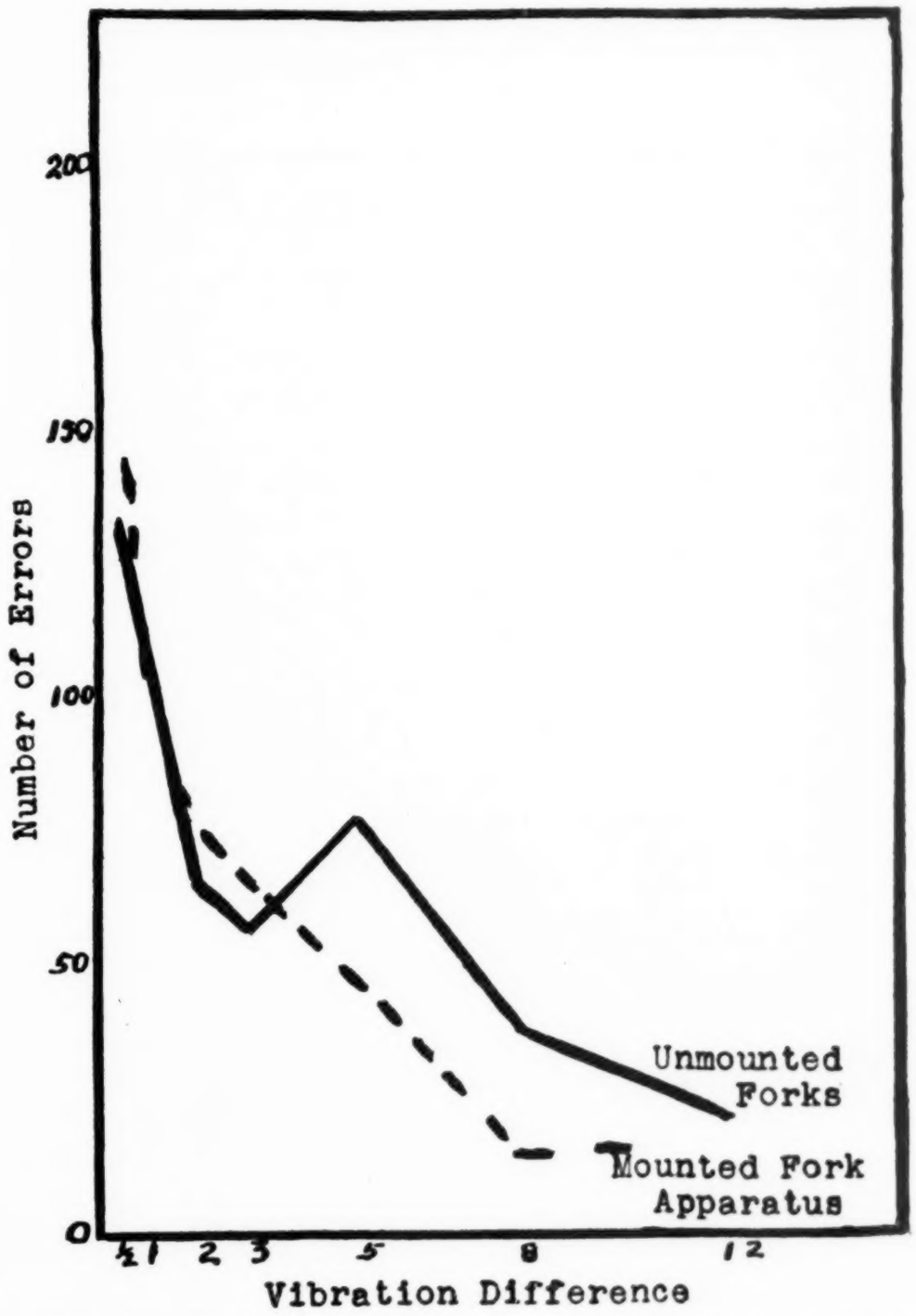
SHOWING PER CENT OF CORRECT JUDGMENTS FOR 30 SUBJECTS IN PITCH DISCRIMINATION USING MOUNTED FORK APPARATUS AND UNMOUNTED FORKS

No. of Cases	Mounted Forks	Unmounted Forks	Advantage Mounted
1	60	66	− 6
2	77	81½	− 4½
3	76	54	21
4	57	66	− 9
5	81½	68½	13
6	79	70	9
7	79	70	9
8	83	70	13
9	70	73	− 3
10	88½	87	1½
11	64	58½	5½
12	81½	81½	0
13	80	91½	−11½
14	86½	83	3½
15	88½	80	8½
16	68½	73	− 5½
17	78½	86	− 8½
18	73	61½	11½
19	47	51½	− 4½
20	84	78½	5½
21	67	70	− 3
22	86	80	6
23	94	90	4
24	93	90	3
25	81½	74	7½
26	93	88½	4½
27	67	75½	− 8½
28	80	81½	− 1½
29	87	90	− 3
30	93	78	15
	78.1	75.6	2.5

RESULTS

Table II. and Graph 2 give the scores for each method. There were 511 errors when the unmounted forks were used and 471 when the apparatus was used, an advantage of 8.3 per cent in favor of the apparatus. But this difference in favor of the apparatus is not as significant as the regularity of the records with the apparatus as compared with the

records for the unmounted forks. The difference in loudness must have been mistaken for pitch difference many times with the unmounted forks. This occurred many times when the difference in pitch was relatively great and must account for



GRAPH II. Showing the number of errors and their relation to the vibration difference of the two tones. 30 subjects.

the greater number of errors in the 5, 8, and 12 v.d. column. This probably sometimes occurred with the apparatus. But practice in striking piano keys soon develops a very regular uniform stroke and hence not so many errors in the columns for the greater vibration differences with the apparatus.

These results prove very conclusively that for exact work, tones for pitch discrimination should be relatively pure tones, with as little quality and intensity difference as possible, that is, the one factor of pitch difference should be isolated for the comparison. The Seashore records have been standardized and are no doubt very satisfactory for the purpose for which they are used in school-room tests. They serve a real purpose in selecting those subjects who can, from those who cannot, discriminate between relatively large pitch differences. But for laboratory experiments on pitch differences the phonograph records are not satisfactory. The unmounted forks set loosely in a small sounding box and struck at different intensities are relatively satisfactory. A much more satisfactory apparatus no doubt could be constructed similar to the one here described with an electro-magnetic device for actuating the hammers. But for practical laboratory tests the results indicate that our apparatus is very satisfactory.

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DISCUSSION

THE STIMULUS ERROR

BY A. P. WEISS

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"This experiment seems to show the futility of properly interpreting the statistical results of a purely behavioristic study without the control of the introspective report" (p. 76), concludes Fernberger[1] in an article that analyzes the types of reactions in a weight discrimination experiment. He bases his conclusions on the fact that: "In the case of lifted weight stimuli, the following instructions are capable of fulfilment; namely, (*A*) judging the intensity of the pressure sensations on the tips of the fingers; (*B*) the intensity of the kinesthetic sensations localized in the wrist; and (*C*) judging, by assuming a stimulus attitude, the relative intensities of the weights themselves. This latter is distinctly a complex perceptual process on the 'meaning' level" (p. 76).

Fernberger implies that the results secured when instructions (*C*) are given are 'purely behavioristic,' and that the evidence for the futility of the behavioristic method is found in the fact that the statistical results from instructions (*A*) and (*B*) differ from each other and from (*C*), and that this difference can only be 'interpreted' through the introspective report. By interpretation Fernberger seems to imply that he has evaluated, from the introspective reports, the different potencies of such factors as, practice, kinesthesis, pressure, visual imagery, widespread tactual and kinesthetic experiences, various degrees of subjective assurance, etc. If only those factors mentioned are considered, his experiment includes the following variables, all of which have an effect on the nature of the discriminating reactions that are released: (1) Pressure stimuli; (2) kinesthetic stimuli; (3) an auditory stimulus "to judge pressure sensations only"; (4) an auditory stimulus to "judge kinesthetic sensations only"; (5) an auditory stimulus to "judge the weights themselves"; (6) practice effects; (7) visual imagery; (8) widespread tactual experiences; (9) widespread kinesthetic

[1] Fernberger, S. W., 'An Experimental Study of the "Stimulus Error,"' J. Exper. Psychol., 1921, *4*, 63–76.

experiences; (10) various degrees of subjective assurance. When a behaviorist tries to investigate such a phenomenon made up of ten variable factors, he tries to keep nine of them constant while varying one factor and measuring the effects on the phenomenon in question. Such an ideal technique is, of course, not possible in a weight discrimination experiment as conducted by Fernberger. The claim that the introspective method makes it possible to approach this ideal, needs to be substantiated. The behaviorist would say that the introspective method merely adds further variables and instead of approaching an ideal technique we get further away from it.

In a behavioristic experiment on weight discrimination the first problem is that of determining what is to be included under the terms "weight discrimination." There are hundreds of different conditions under which weights may release the verbal reactions of *lighter*, *equal*, *heavier*. Which combinations of conditions are to be investigated? Unless special theoretical considerations are involved, the behaviorist would probably select those conditions under which weights are *usually* discriminated by scientists, post-office clerks, store-keepers, housewives, etc. This would give a scientists' limen, a post-office clerks' limen, a store-keepers' limen, a housewives' limen, etc. Which is the *true* or psychological limen? This question can only mean: in which of these limens is the investigator most interested? One is just as true or just as psychological as the other. No behaviorist would deny that a number of different limens may be secured even from the same individual at different times. Further, these limens may be given different names such as stimulus attitude, pressure attitude, kinesthetic attitude, etc., but this only means that the ten factors in weight discrimination mentioned in a preceding paragraph do not all have the same potencies even in the same individual at different times. Instead of dividing the weight-discriminating limens into scientists', post-office clerks', store-keepers', and housewives' categories, the behaviorist might have set up the special condition that the subject must be a graduate student in a psychological laboratory trained in introspection, and trained to discriminate *attitudes* in addition to discriminating weights. This would of course give a still different set of limens. To meet certain theoretical demands one might also reserve the term *psychological limen* for that limen secured under the conditions known as the 'process attitude.' To imply however that this is the limen which

the behaviorist *must* regard as the *true* limen of weight discrimination, will hardly be taken seriously by any behaviorist. To maintain that this limen can only be interpreted from a consideration of the introspective reports is, of course, perfectly true, since the introspections are part of the total reaction that is being investigated. If in addition to requiring introspections the experimenter had required the subject to sing an anthem while the experiment was in progress, the way in which these anthems were sung would also have to be considered in the interpretations.

From the behaviorist's standpoint Fernberger's experiment in which *attitudes* are introduced, is not an experiment in weight discrimination at all. It is an experiment to determine whether or not a subject can or cannot "abstract sensation," whatever this may mean. The weight discrimination reactions are merely used as a device for measuring the extent to which *abstraction* is possible. Many other types of reactions (visual, auditory, olfactory, etc.) would have served equally well. The behaviorist is fully aware of the fact that to get some specific limen, it is necessary to maintain the stimulus conditions and the physiological conditions as constant as possible. He does not believe that introspection reveals the extent to which these conditions have remained constant.

A behavioristic investigation is usually concerned with such things as the classification of behavior, the effect of inheritance or training on behavior, the effect of the physical or social environment on behavior; behavior being regarded as those movements of the organism that establish its social (anthropological) status. If introspections are going to prove valuable in investigations directed along these lines, the behaviorist will use them. The present writer regards introspections as verbal reactions that have been developed under conditions quite foreign to the conditions under which normal human reactions occur. They are abnormal supplementary speech reactions of the same nature as illusions, dreams, etc. That the conditions under which they occur may be standardized and the reactions themselves made relatively stable, he would not deny.

The direct response to Professor Fernberger's charge of the 'futility of properly interpreting the statistical results of a purely behavioristic study,' would be:

1. None of Fernberger's experiments are 'purely' behavioristic in the sense that he is trying to establish the laws according to which weight discriminations are made by some social or anthro-

pological unit. They are behavioristic in the sense that he has investigated the manual and speech reactions of three individuals reacting under conditions involving at least ten variables.

2. By introducing the modification of establishing an *attitude* he has made the attempt of measuring two simultaneous variables (pressure and kinesthesis) in terms of a third variable (introspection) much less uniform than either the pressure or the kinesthesis.

3. The introduction of the *attitude* conditions changes the experiment from one of normal weight discrimination to what he would call an experiment on the 'abstraction of sensations.'

4. The behaviorist would like to know just what is to be included under 'to properly interpret?' If to interpret means that the introspections reveal the presence of mysterious *mental* processes that are not physiological, the behaviorist frankly admits his futility. If to interpret is to mean that the introspective reactions are to be regarded as supplementary speech reactions which occur in all adults and as such form part of the total reaction to the weights, the behaviorist will admit this also. He would affirm, however, that the introspective reaction as an end result is hopelessly inadequate to reveal the inheritance factors, the environmental factors, and the training factors, which are the causes of a given set of reactions.

5. Finally, the habit of capping off an experiment in introspection with the dogmatic statement that "therefore the futility of the behavioristic method is clearly shown" is somewhat unfair. The behavioristic method is futile only when it fails to solve *its own problems*. It is not futile because it does not seem to solve the special problems of the introspectionist.

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